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CONTENTS

ORIGINAL PAPERS

The Changes in Refraction Between the Ages of 5 and 14—Theoretical and Practical Considerations. Monroe J. Hirsch 445

The Common-Sense Horopter. Gordon L. Walls 460

Suggestions for a New Fee Structure in Optometry. Part III. Carel C. Koch 478

EDITORIALS

Optometry Welcomes Sheard at Los Angeles College 490

Many Metropolitan Areas Need Optometrists 492

TRANSACTIONS OF THE ACADEMY 493

CURRENT COMMENTS 494-498

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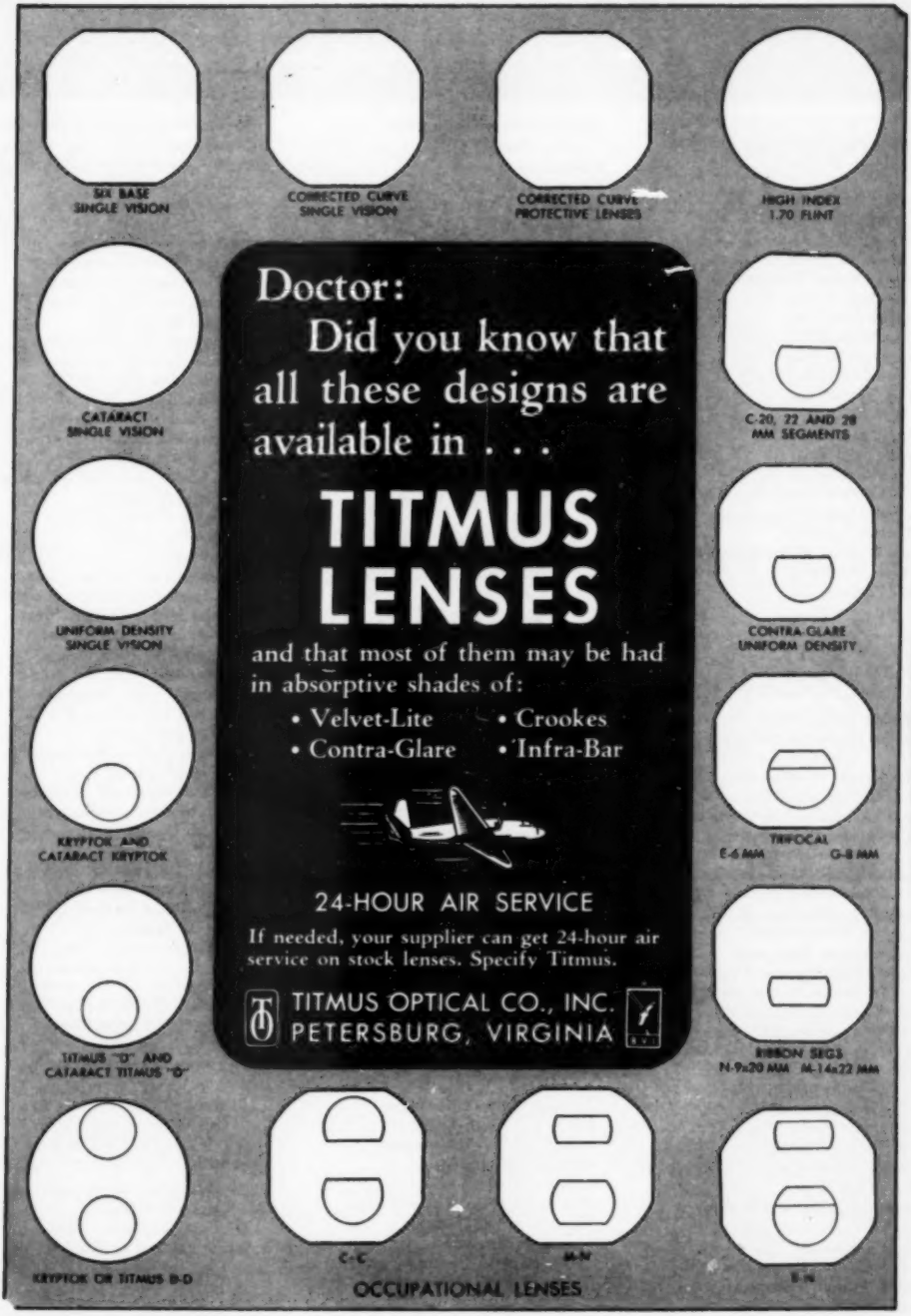
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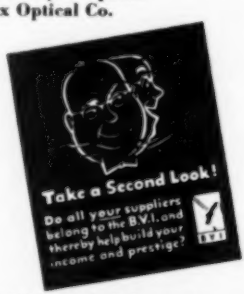

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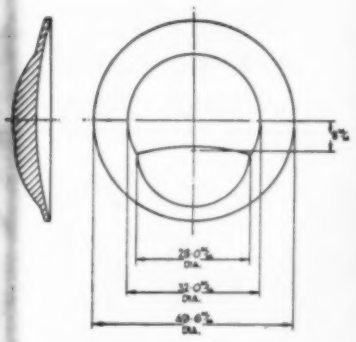

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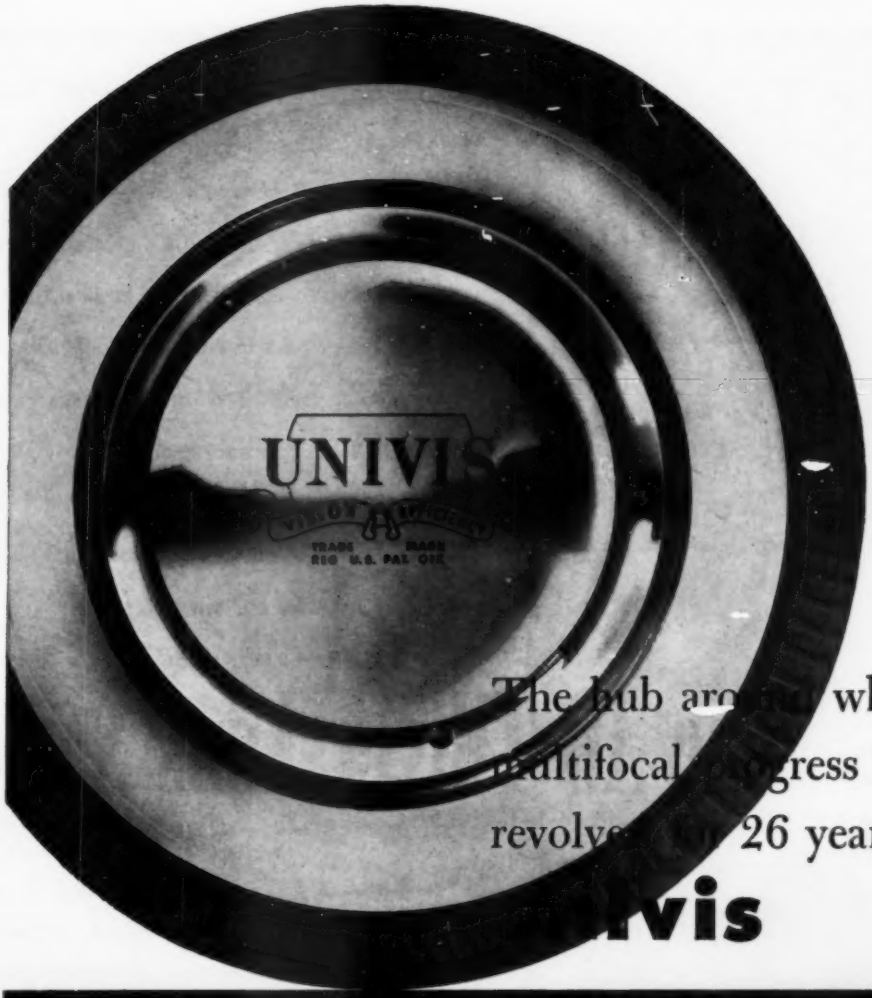

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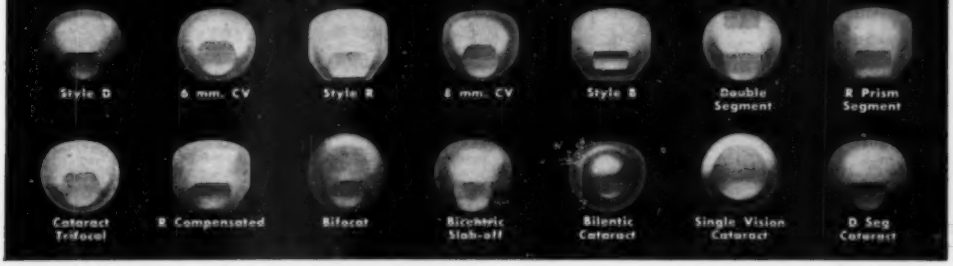

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fig. 1.

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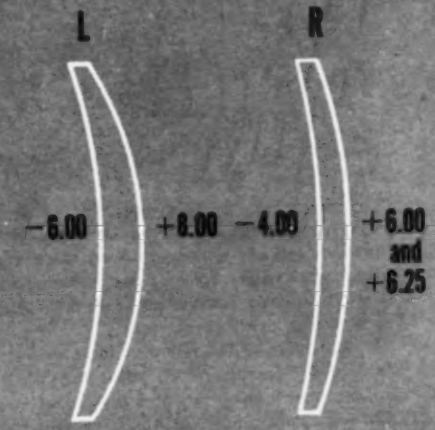

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fig. 2.

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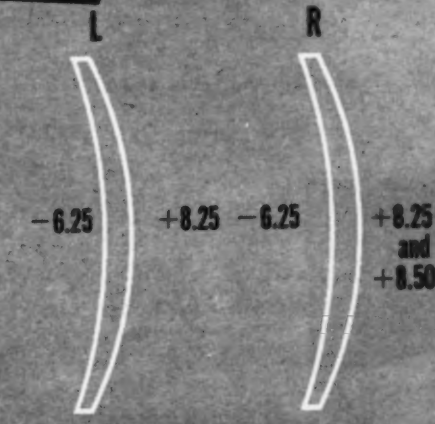

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THE CHANGES IN REFRACTION BETWEEN THE AGES OF 5 AND 14 — THEORETICAL AND PRACTICAL CONSIDERATIONS*

Monroe J. Hirsch†
Los Angeles College of Optometry
Los Angeles, California

Since the publication in 1867 of the results of the examination of the eyes of 10,060 school children in Breslau by Hermann Cohn[1] a number of similar studies have been reported. While these studies are not substitutes for the much needed and still undone longitudinal study (the same children examined periodically for a number of years) they have merit and have furnished much of the present day knowledge about the nature and development of refractive errors. In order for an investigation of the refraction of a group of children to be of maximum value, four conditions should be met:

a) *The sample should be randomly selected.* This condition must be met if conclusions are to be drawn concerning the nature of refraction. The major weakness of the studies by E. V. L. Brown[2] and Slataper[3] is the fact that their data are representative of clinical and private practice; as a result the conclusions, while enticing, must be accepted with reservations. Patients seeking help for ocular defects are not representative of the population, but rather comprise a selected group.

b) *The sample should be of considerable magnitude.* Ametropia, and particularly myopia, is present in only a very small percentage of the cases, especially in young children. Hence the random sample must be large so that a sufficient number of ametropes may be found. The study of Kempf, Jarman and Collins[4] excellent in all other respects, suffers from this defect.

c) *The method of measurement of refraction should be both reliable and valid.* The method of choice is skiametry, with accommoda-

*Read before the annual meeting of the American Academy of Optometry, New York, New York, December 10, 1951. For publication in the September, 1952, issue of the AMERICAN JOURNAL OF OPTOMETRY AND ARCHIVES OF AMERICAN ACADEMY OF OPTOMETRY.

†Optometrist, Ph.D. Member of faculty. Fellow, American Academy of Optometry.

tion, in some fashion, relaxed. Today only slight reliance can be placed upon 19th century studies, since the refraction was only estimated, usually by means of the ophthalmoscope.

d) *Appropriate statistical methods should be used.* Perhaps the greatest shortcoming of most of the former studies is the inappropriateness of the statistical methods which were utilized. It is only in the very recent past that the science of statistics has become known to those engaged in research on the eye and that the more subtle techniques have been applied to refractive data. Because the distribution curve for refraction is both leptokurtic (peaked more than a normal curve) and skewed (asymmetrical) measures like the mean and standard deviation which are useful in describing the normal curve are of limited value; techniques which yield more information must be sought.

If a study meets the four conditions just enumerated, two types of information may be gained. First, and of practical importance, the "normal" values for the refraction of children of various ages may be learned. The optometrist who has children as patients is well aware of the need for such data. Of perhaps greater importance, however, is the second type of information, that concerning the nature of refractive errors. If the rate of change of refraction at various ages were known, the search for the causal factors in the genesis of myopia and hyperopia would be aided.

The present paper is a report of the retinoscopic determination of the refractive state of 9,552 randomly selected school children. The four conditions just enumerated have been adhered to as closely as possible, and an attempt will be made by statistical treatment to glean as much information as the data will yield about the practical and theoretical aspects of the problem.

METHODS:

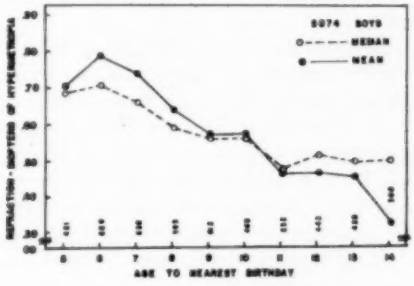
The measurements of refractive state were incidental to a visual survey in a number of towns in the vicinity of Los Angeles, California. Since all of the pupils were examined, the sample represents an unselected group of elementary school children. Skiametry in each of the two major meridians was performed by the author in about 25 per cent of the cases, a clinical instructor in charge of surveys in about 25 per cent of the cases, and a student* in the remaining half of the cases. The method, previously described by the present author,[5] enables accommodation to be relaxed physiologically.

*The students who performed retinoscopy in this study had already completed one and one-half years of clinical practice and had each performed retinoscopy on children by this method during a preceding year at which time under the supervision of an instructor they had completed more than 100 eyes.

For the present statistical treatment only the right eye is considered, and the refractive state in the two major meridians is averaged (equivalent sphere). The basic data for the present study, therefore, consist of the spherical equivalent refractive state of the right eye for 9,552 randomly selected school children between the ages (nearest birthday) of five and fourteen years. Because there is some evidence that sex differences in refraction may exist, the data for boys and girls are treated separately throughout the study.

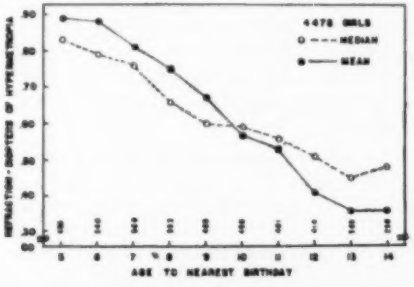
RESULTS:

The mean and median for each year were calculated for the boys and for the girls. These values are presented in Figures 1 and 2. Both the mean and the median measure central tendency, and as is seen, both are similarly affected by increased age. The average refractive error is hyperopia throughout the period under study, and, as is seen, the amount



of hyperopia decreases steadily with age. There are, however, two less obvious points which should be discussed:

1) It will be noted that for the boys, both the mean and median at five years of age is lower than the similar measures for the sixth year.



This would suggest that the average degree of hyperopia is rising prior to the sixth year and reverses at that time. While the same phenomenon is not as clearly demonstrated for the girls, it will be noted that the slope is less steep for the mean between the fifth and sixth year than later, and less steep for the median between the fifth and seventh year than for a later period. Were it not that such a phenomenon has been suggested by the work of other authors, this might be regarded as a chance variation. However, a curve rising until the sixth or seventh year and then declining has been suggested by the studies of E. V. L. Brown and of Slataper. It was stated earlier that these works suffer from being clinically selected material. The present random data, however, serve to substantiate their theory. While the picture is still not clear, there is sufficient evidence now to suggest that a careful study of a group of pre-school children chosen at random may prove fruitful, and may conclusively disprove the old notion that the average refraction proceeds toward less hyperopia from birth onward.

2) The figures indicate that for both boys and girls the slope of the line connecting the medians is considerably less steep than that for the means. For the younger children, the mean is of greater magnitude than the median. With increasing age, this difference decreases until the two measures are the same at the age of between nine and eleven. After this, the mean is of lesser magnitude, the difference increasing with age.

For the normal curve the mean and median are identical. A difference between these two measures indicates a lack of symmetry, or the presence of skewness. The refractive curve is shown by this comparison to be skewed toward the hyperopic side in early school years, symmetrical at about the age of ten, and then increasingly skewed toward the myopic side with increasing age. This finding is also contrary to the frequently held view that the curve for refraction is normal at birth and during growth becomes increasingly skewed toward the myopic side. If the figures for the newborn given by Wibaut[6] are accepted,* then we must imagine that the refractive curve is normal at birth, becomes skewed to the hyperopic side during the pre-school years, then begins becoming skewed in the other direction, reaching symmetry again at about age ten, and continues until it reaches the degree of negative skew characteristic of maturity.

This difference between mean and median is important when the practical question of "normals" for each age is considered. For the boys the mean decreases 0.39 D. between the age of five and fourteen. How-

*There is some justification for regarding the Wibaut data with caution since the curve he presents is a composite of the findings of four authors, whose data are not homogeneous. This is discussed elsewhere by the present author and Frank W. Weymouth.

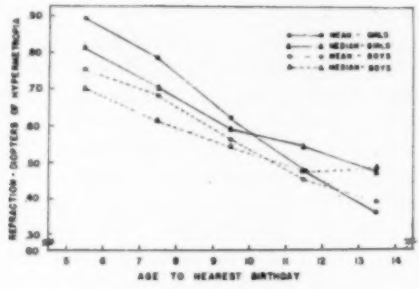
ever, during this same period the median decreases just about half this amount or 0.20 D. For the girls the change in mean between the youngest and oldest age studied is 0.53 D., while the median only declines 0.35 D. What then can be said about the "expected refractive change" for the individual?

Changes of this sort can have only one interpretation, namely, that there is little change (about 0.25 D.) for most of the children, while a few change more markedly. The greater change in the mean is due to the extremes, the high hyperopes among the younger children and the increasing number of high myopes among the older children. This effect can best be demonstrated by a simple example. If we have five children at some young age, whose refractive states are +0.25, +0.50, +0.75, +1.00, and +2.50 D., then the mean is +1.00 D. and the median is +0.75 D. This is the situation in the younger children, as is seen from the figures, i.e., the median shows less hyperopia. It will be seen from the examination of the figures in the example that this difference is for the most part due to the presence of the single high hyperope. Now, as these children grow older, let us assume that the refraction of four of the children is not changed, but that the one which was formerly +0.75 D. becomes a myope, and by the age of fourteen has a refractive error of —3.00 D. The mean refractive state will now be +0.25 D., while the median is +0.50 D. Thus, comparing the two ages, it is seen that the mean changed from +1.00 to +0.25, while the median changed from +0.75 to +0.50. This is essentially the sort of change which the figures show. However, it will be noted that in so far as the individual is concerned, four of the children in the example remained stationary, the shift in mean and to a lesser degree in median being due to the one child who became myopic.

The example cited above is not atypical, and illustrates one of the difficulties inherent in any concept of "normal" or expected. In the field of refraction it has been customary to study means at various ages and to attempt to predict what an individual will do on this basis. This is obviously a poor procedure, and a more satisfactory procedure will be described subsequently.

In order to compare the data obtained for the boys with that for the girls, it is desirable to have a more regular curve. The data were recalculated for two-year steps. Thus, the five and six year olds were grouped together and considered as a group whose age ranged from four and a half years to six and a half years, and whose average age was five and a half years. Similarly the seven and eights were grouped together, etc. Each curve therefore is based upon five larger

groups instead of 10 smaller groups as in Figures 1 and 2. These data, i.e., the mean and median for two-year steps for both boys and girls, are presented in Figure 3.



In Figure 3, both mean and median are higher for girls than for boys during most of the age range under investigation and it is only for the oldest age group (thirteen and fourteen years old) that the boys exhibit greater hyperopia or less myopia. The average differences, however, while clearly real are not of great magnitude. In general, it may be stated, that the average refractive state (either mean or median) at the age of five or six is about an eighth of a diopter more hyperopic for the girls than it is for the boys. On the other hand, at the upper age limit here studied, both mean and median for boys and for girls are essentially the same. Thus, during the nine-year period here studied, the change in refraction of the girls exceeded that of the boys by about an eighth of a diopter or somewhere between 0.01 and 0.02 D. per year. While this difference is probably real, it is so small as to be of no practical importance in establishing "norms" for the various ages. This slight difference may, however, be of some theoretical importance, a fact which will be discussed subsequently.

In order adequately to describe a frequency distribution, it is necessary to have some measure of dispersion as well as a measure of central tendency. Since the distribution of refraction is not normal, the customarily employed standard deviation suffers from serious shortcomings. For this reason, the less frequently used, but in this case more meaningful percentiles, were calculated.

The data were grouped in two-year intervals, five-six being the lowest and thirteen-fourteen being the highest. For each group, the cumulative frequency distribution was obtained, and the cumulative

frequency ogive was plotted. These figures are not presented here, but certain of the data extracted from is given in Table I. The figures in

TABLE I

Refractive States of certain percentages of the children of various ages:

Ages	Sex	PERCENTAGES OR PERCENTILES 7	25	50	75	93
5 - 6	Boys	—0.01	+0.33	+0.60	+0.87	+1.23
7 - 8	Boys	—0.11	+0.25	+0.50	+0.79	+1.13
9 - 10	Boys	—0.24	+0.13	+0.41	+0.69	+1.12
11 - 12	Boys	—0.50	+0.08	+0.35	+0.63	+0.99
13 - 14	Boys	—0.79	+0.03	+0.34	+0.62	+0.99
Net Change*	Boys	0.78	0.30	0.26	0.25	0.24
5 - 6	Girls	+0.02	+0.39	+0.68	+0.95	+1.56
7 - 8	Girls	—0.07	+0.29	+0.59	+0.92	+1.53
9 - 10	Girls	—0.37	+0.20	+0.48	+0.77	+1.39
11 - 12	Girls	—0.73	+0.07	+0.34	+0.72	+1.22
13 - 14	Girls	—0.81	—0.01	+0.33	+0.65	+1.07
Net Change*	Girls	0.83	0.40	0.35	0.30	0.49

*Net Change between the youngest and the oldest age group.

the body of the Table are in diopters, and myopia (negative correction lens) was placed at the lower end of the cumulative distribution. A few examples will make the Table meaningful: The first number in the body of the Table indicates that seven per cent of the group of five or six year old boys had a retraction of —0.01 D. or less (i.e., more myopia). For the thirteen and fourteen year olds, seven per cent had a refraction of —0.79 D. or less. Reading across the first line it is seen that 25 per cent of the five and six year old boys have a refraction of +0.33 D. or less. Furthermore, it is permissible to subtract seven from 25 and conclude that 18 per cent of the boys in this age range had refractive states between —0.01 and +0.33 D. In this fashion the various ranges were obtained, and the more important of these are presented in Table II.

The choice of the 7th, 25th, 50th, 75th, and 93rd percentiles was made after careful consideration. The 50th percentile is of course the median. The 25th and 75th percentiles include between them the middle 50 per cent of the cases. The range between the 7th and 93rd percentiles was described by Truman L. Kelly[7] as a measure of dispersion which varies less from sample to sample than any other range, i.e., it is the most stable of the inter-percentile ranges. Furthermore, some value smaller than 10 per cent was needed if the changes at the myopic end of the curve were to be appraised.

The answers to many of the questions of a clinical nature which optometrists frequently ask can be answered by referring to Table I.

The simple question "What is the average refraction for a fourteen year old boy?" may be answered by referring to the column of medians (50th percentile), and is found to be +0.34 D. In addition, much more astute observations about refraction for this age group may be made. For example, it may be stated that the middle 50 per cent of this group (thirteen-fourteen year old boys) have refractions ranging from +0.03 to +0.62 D. Furthermore, with rough interpolation, further statements may be made. If the need for any minus lens be considered as myopia, then it will be seen that among the five and six year old boys, about seven per cent have myopia, while among the oldest age group here considered, about 24 per cent have myopia. (Rough interpolation based upon the fact that 25 per cent have refractions of +0.03 D. or less, while seven per cent have refractions of —0.79 D. or less.) The optometrist will find the answer to many of the questions as to the frequency of the various refractive states embodied in the Tables.

The change between the youngest age group (five-six) and the oldest (thirteen-fourteen) are presented in Table I and are of considerable theoretical importance. If as children grew the refractive distribution curve were shifted toward the myopic side, then one would expect the change to be the same for each percentile group. This, however, is not the case, and as is seen for the boys the change is about 0.25 D. for all the percentiles except the lowest (7th percentile) which manifests a change of 0.78 D. For the girls, the change is close to 0.35 D. for all percentile groups except the seventh which is more than double this amount, i.e., 0.83 D. This can be explained in only one way, and that is that there are different rates of growth for different individuals. The concept that all children grow toward myopia, and that the child who is myopic at the age of fourteen is merely the one who attained emmetropia at an age when others were hyperopic is, in part at least, erroneous. Seemingly, most children change very little during the age range here studied. Most boys become only 0.25 D. less hyperopic, and most girls about 0.35 D. less hyperopic. On the other hand, a small number of children changed by more than this amount and it is these which lead to the development of the myopic tail of the curve for adults.

A number of interesting facts are brought out by the ranges considered in Table II. It is seen from column 4 that at no age does the middle 86 per cent of the cases exceed 2.00 D. in range. This illustrates the remarkably small degree of variation in refraction for the majority of the cases. The practitioner who deals primarily with the extremes

TABLE II

Range in Diopters for various percentages.

Age	Sex	(1) 43 per cent below median (7th to 50th percentile)	(2) 43 per cent above median (50th to 93rd percentile)	(3) Middle 50 per cent (25th to 75th percentile)	(4) Middle 86 per cent (7th to 93rd percentile)
5 - 6	Boys	0.61	0.63	0.54	1.24
7 - 8	Boys	0.61	0.63	0.54	1.24
9 - 10	Boys	0.65	0.71	0.56	1.36
11 - 12	Boys	0.85	0.64	0.55	1.49
13 - 14	Boys	1.13	0.65	0.59	1.78
5 - 6	Girls	0.66	0.86	0.56	1.54
7 - 8	Girls	0.65	0.94	0.63	1.60
9 - 10	Girls	0.85	0.91	0.57	1.76
11 - 12	Girls	1.07	0.88	0.65	1.95
13 - 14	Girls	1.14	0.74	0.66	1.88

often loses sight of the narrow range within which most of the population fall. The interpretation of this column may be illustrated as follows: For the youngest boys, 86 per cent of the children do not vary from each other (in so far as refraction is concerned) by more than 1.24 D. This is of course the difference between the highest and lowest refraction of the 86 per cent, and therefore most cases vary still less. For example, as is seen from column 3, 50 per cent of the cases vary less than 0.54 D.

As might be expected from our knowledge of other biological data, the variability increases with age. Thus with increased age we have the appearance of greater numbers of extremes, and the range in column 4 is seen to increase both for the boys and girls. Striking, however, is the small amount of this increase. The fact that column 4 (middle 86 per cent of the cases) increases in magnitude more than does column 3 (middle 50 per cent) which hardly changes at all with increased age, indicates increasing leptokurtosis. The ranges in column 4 are the sum of those in columns 1 and 2, and it will be noted that the increased variability with age is primarily due to changes in column 1, i.e., the appearance of myopes.

Comparing the data for boys with those for the girls, it is seen from column 4 that the girls are more variable at each age than are the boys. Examination of columns 1 and 2 reveal the fact that for the most part this is due to the differences in column 2, for the first two age groups, and to both for the next two age groups. Thus the girls are more variable at the younger ages because there are among them more hyperopes: they are more variable at ages nine to twelve because there is a greater number of both types of extreme than among the

boys: there are less differences between the sexes at the oldest ages because of the sharp increase in column 1 for the boys (more myopes). This indicates an increase in myopia at an earlier age among girls than among boys, a fact brought out by most random data.

The difference between columns 1 and 2 is an approximation of the skewness of the distribution. Thus, at the younger ages the skew is toward the hyperopic side, and is greater for the girls than for the boys. At the older ages the skewness is in the other direction (long tail on the myopic side) and is of almost equal magnitude for the two sexes. This shift of skewness from positive to negative has already been discussed.

The data in Tables I and II may be presented in another fashion. Percentages may be included in the body of the table, and refractive state may be used as column headings. This is the scheme in Table III.

The low variability previously discussed is clearly demonstrated in Table III. For all age groups, between 66 and 77 per cent of the cases

TABLE III

Percentage of cases having various refractive states.

		MYOPES				HYPEROPES			
Age	*Sex*	*over 3*	*2 to 3*	*1 to 2*	*0 to 1*	*0 to 1*	*1 to 2*	*2 to 3*	*over 3*
5 - 6	Boys	0.15	0.18	0.34	6.76	77.43	13.14	0.86	1.14
7 - 8	Boys	0.16	0.33	0.41	10.12	77.14	9.06	1.15	1.63
9 - 10	Boys	0.31	0.69	0.82	13.86	75.66	5.83	1.55	1.28
11 - 12	Boys	0.26	0.66	2.16	17.66	73.00	4.72	0.41	1.13
13 - 14	Boys	1.37	0.83	2.88	17.45	71.01	4.81	0.65	1.00
5 - 6	Girls	0.00	0.20	0.25	5.70	72.62	16.61	3.18	1.44
7 - 8	Girls	0.23	0.22	0.53	8.73	71.22	14.70	2.50	1.87
9 - 10	Girls	0.25	0.49	1.27	15.17	67.97	11.67	1.68	1.50
11 - 12	Girls	1.35	1.72	2.70	15.83	66.25	9.84	1.33	0.98
13 - 14	Girls	1.61	1.92	2.25	19.58	66.13	5.94	1.77	0.80

have a refraction of between zero (emmetropia) and +1.00 (1.00 D. of hyperopia), while for all age groups more than 90 per cent of the children have refractions within the range of —1.00 to +2.00 D. Other characteristics of the distribution which are brought out by this table may be summarized as follows:

a) The percentage of hyperopes above 2.00 D. is decidedly higher for the girls than for the boys. With increasing age this difference tends to disappear, but even at the oldest age there is a difference.

b) The percentage of hyperopes above 2.00 D. decreases with age, but not very rapidly. The numbers being small, it is dangerous to make too close predictions, but it seems reasonable to state that more than half of the hyperopes of more than 2.00 D. will remain in this range throughout life. This is of considerable importance clinically since there is a tendency among many clinicians to assure parents that

a highly hyperopic child will outgrow the anomaly. Unfortunately, this is not the case, as the data indicate, and as comparison of these figures to those for adults clearly demonstrates. There is a decrease in this group, but less than half of the children with considerable hyperopia at the age of five will have "outgrown" it by the age of fourteen . . . or even by adulthood.

c) There is a sharp decline in the percentage of hyperopes between 1.00 and 2.00 D., there being approximately 1/3 as many of these at the age of fourteen as at the age of five. Thus a prediction of reduction in moderate degrees of hyperopia has a two out of three chance of being correct. It should be realized that part of this effect is an artifact, consequent upon the scale (grouping of cases, etc.). However, from a practical point of view, predictions should be made in accord with the Tables. If a child of five or six years has more than 2.00 D. of hyperopia as measured by non-cycloplegic refraction, the odds are in favor of his always having a refraction in excess of 2.00 D., and hence probably always wearing glasses. On the other hand, if the refraction is between 1.00 and 2.00 D. of hyperopia, the chances seem to be very good that by maturity, the error will be less than 1.00 D. and in all probability optical correction will either not be worn, or worn occasionally. Needless to say, the prediction is better at the lower end of the group (hyperopes of 1.00 and 1.25 D.) than at the upper end (hyperopes of 1.75 and 2.00 D.).

d) The percentage of children between zero and 1.00 D. of hyperopia is very high, and does not change markedly with increasing age. This seems to be higher by about five per cent at all ages for boys than for girls. If the old concept of *emmetropization* is followed, it would seem that whatever the mechanism is, it is more pronounced in the male between the ages of five and fourteen.

e) The percentage of low myopes increases markedly for both sexes, being about three times greater at the age of fourteen than at the age of five. From experience in clinics, it is known that this figure will continue to change considerably up to the period of early adulthood, and hence no conclusions seem warranted here, other than that there is a marked increase in this group during this period of life.

f) Myopes above 1.00 D. in magnitude (refractions of less than —1.00) also increase markedly during this period. To better understand this group the first three columns of Table III may be combined. This is done in Table IV, where the numbers in the first two columns are the percentage of children having refractions of 1.00 D. or more of myopia. The third column in Table IV enables comparison of girls

TABLE IV
Percentage of children with myopia in excess of 1.00 D.

Age	*Girls*	*Boys*	*Ratio**
5 - 6	0.45	0.67	67
7 - 8	0.98	0.90	109
9 - 10	2.01	1.82	110
11 - 12	5.77	3.08	187
13 - 14	5.78	5.08	114

*Number of myopic girls per 100 myopic boys in a population of equal number of each sex for each age group.

and boys to be made, and is the number of myopic girls per 100 myopic boys. Columns 1 and 2 clearly demonstrate the increase in myopia with age: at the age of five or six, myopia of more than a diopter is rare, being present in only about half a per cent of the children. On the other hand, between five and six per cent of the thirteen and fourteen year olds have this refraction. This figure will undoubtedly increase somewhat between the ages of fourteen and twenty.

As is seen in column 3, of Table IV, there seem to be more myopic boys than girls at the youngest ages. However, due to the very small numbers, this observation must be accepted with caution. For the age range of seven to ten, the number of myopes is about the same for both sexes. In the eleven to twelve age range, a large difference exists, there being 187 myopic girls for every 100 myopic boys, or close to a two to one ratio. At the age of thirteen to fourteen, this ratio returns to close to a 1:1. This can only be interpreted as indicating a marked increase in myopia of more than 1.00 D. in girls at the ages eleven and twelve, and in boys at the ages thirteen and fourteen. This same phenomenon, namely the development of myopia in girls preceding that in boys by about one or two years, and the marked rise in each sex coinciding with the period of puberty, has appeared in all of the data that the present author has examined or accumulated, and must assume an important role in any discussion of the aetiology of myopia. It should be noted in passing that in so far as body size is concerned, there is general agreement that a period of rapid growth or "second stretch" occurs at about this time of life. Martin[8] for example, giving the ages of ten to fourteen for girls and twelve to sixteen for boys as periods of rapid growth. The two year differential and the similar ages in the development of myopia is suggestive, and worthy of consideration.

Trends or changes in average of the group and changes in variability or dispersion have been discussed. The interesting question of individual differences now should be considered. While final and con-

clusive evidence on refractive state changes in individual children can only come from a longitudinal study, some information can be obtained from the data here being considered. From Tables I, II, III, and IV, certain preliminary statements may be made.

a) For the five-year olds myopia of more than 1.00 D. is rare, while for the fourteen year olds about two per cent of the cases have myopia of more than 2.50 D. Thus it may be concluded immediately that at least two per cent of the children change in refractive state by more than two diopters. The change may be more than this in some cases, since the sample included some four diopter myopes, but these figures may be set as a minimum.

b) Despite the shift of anywhere from 1 D. to 3 D. or 4 D. toward myopia on the part of this small percentage of the cases, the median shifts only about 0.25 D. to 0.37 D. The bulk of the cases therefore must change by approximately this amount. Thus the small shift in the median indicates a small change in the majority of the cases, while the development of a myopic tail on the distribution curve for older children indicates that some children change considerably. The mean affected by these two different events is merely a reflection of their relative weights, and hence is seen to be a very poor measure for predicting what an individual child will do.

c) The hyperopic tail does not decrease as rapidly as the myopic tail develops. Comparison of myopes and hyperopes of more than 3.00 D. magnitude in Table III reveals this very clearly.

Since the distribution curve is known for the five year olds, and for the fourteen year olds, the following problem might be set up:

How much would each five year old have to change in order to obtain a curve like that for the fourteen year olds?

This problem has no solution, unless two assumptions are made, namely that the individuals will retain the same order in the distribution and that change is always in the direction of more myopia or less hyperopia. Thus, if it is assumed that the child who is most myopic at age five will also be most myopic at fourteen years, and the highest hyperope at the youngest age will also be the highest at the oldest age, then the problem has a simple solution. While the assumption is undoubtedly not valid, it is probably not too far from the truth, and the information obtained will be useful until more exact figures result from a longitudinal study.

Table V is a summary of the figures obtained by mathematical solution of the above problem. While these figures are not exact due to the assumptions made, they are probably not far from the true figures for the population. Most striking is the fact that 63 per cent of the

TABLE V

Estimated degree of change necessary to obtain the 13-14 year old distribution based upon the 5-6 year old distribution.

DEGREE OF CHANGE	PERCENTAGE OF THE CASES	
	Boys	Girls
Less than 0.25 D.	63	0
0.25 to 0.50 D.	25	75
0.50 to 1.00 D.	7	18
More than 1.00 D.	4	5
Doubtful	1	2
	100	100

boys change by less than 0.25 D. over this period, while 88 per cent of the boys and 75 per cent of the girls change less than 0.50 D. That the change is greater among girls than among boys is also clearly demonstrated.

This table will have some value in predicting what change a child of five may be expected to make in so far as refraction is concerned during the next nine years. For example, a five year old boy chosen at random has 63 chances in 100 of changing less than 0.25 D.: there is a 25 in 100 chance that he will become between 0.25 D. and 0.50 D. more myopic. Finally there are only 4 chances in 100 that the change will be more than 1.00 D.

SUMMARY:

On the basis of the retinoscopic examination of 9,552 randomly selected school children between the ages of five and fourteen, the following general conclusions may be drawn with respect to refraction during this period of life.

A—The mean refractive state decreases in an approximately linear fashion, going from about +0.90 D. to +0.40 D. for girls and from between +0.70 D. and +0.80 D. to +0.30 D. for boys.

B—The median refractive state changes in the same fashion, but to a lesser extent, indicating that the change in average (mean) is unduly weighted by the few cases which change markedly.

C—The distribution of refraction is skewed toward the hyperopic side until the age of nine or ten, is symmetrical at this age, and then becomes increasingly skewed toward the myopic side with increased age.

D—At all ages investigated, the range including the middle 50 per cent of the cases is almost the same, and is between 0.54 D. and 0.66 D. The range for the middle 86 per cent of the cases increases from about 1.25 D. for the youngest boys to 1.75 D. for the oldest group, and from 1.50 D. to 1.87 D. for the girls. Thus, variability a) is slightly greater among girls than among boys and b) increases between 0.37 D. and 0.50 D. during the age span investigated. Re-

markable is the narrow range within which most of the cases fall.

E—Less than one per cent of the children in the five and six year groups have myopia in excess of 1.00 D., while more than five per cent of the thirteen and fourteen year olds have this anomaly. The increase in number of myopes seems to occur at about the time of puberty, and seems to occur a year or two earlier in girls than it does in boys (as does puberty).

F—The higher hyperopes decrease in number with increasing age, but not as sharply as the number of myopes increases.

G—A change like the one observed between the ages of five and fourteen can best be explained at present, on the basis of a very small change (0.25 D. for boys and 0.37 D. for girls) in the direction of more myopia or less hyperopia on the part of the majority of the children (over 75 per cent); a change of from 0.50 D. to 1.00 D. on the part of between 7 and 18 per cent of the children, and a change of more than 1.00 D. (and up to 3.00 D. or 4.00 D.) on the part of about five per cent of the children.

H—The claim of both Brown and of Slataper that hyperopia increases to the age of six or seven and then begins decreasing is to some extent verified.

I—The study is intended to enable intelligent predictions to be made by the optometrist until the much needed long term longitudinal study can be performed upon a large random sample of children. It is not a substitute for such a study.

J—The study points to two areas where future research must take place—namely the pre-school age (to finally confirm Brown's theory) and the high school age (fourteen to eighteen) where further increases in myopia may be anticipated.

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THE COMMON-SENSE HOROPTER*

Gordon L. Walls†
School of Optometry, University of California
Berkeley, California

The keynote of this workshop is *training*. The great importance of vision is that it affords orientation toward space and its contents. The best excuse I could have for taking up your time would be that I have a suggestion to offer regarding something which you might attempt to train—which, if it is trainable, should improve your patient's orientation in space and toward the visual objects it contains. I do have such a suggestion. I expect that it will seem brand new to you. This means that I shall propose the training of a visual function which no one has ever tried to train, or even thought to be trainable, or seen any value in training.

Orientation in three-dimensional space, when we atomize its tangible basis in the contents of the retinal images, turns out to be a matter of the perception ("correct" or not) of the directions of points and the distances of points. If my visual space is temporarily bounded by the walls of a room, my position in the room is obviously given me by the apparent distances of the several walls, the apparent directions of the door and windows, etc. If I have been made dizzy and these apparent distances and directions are in a state of flux, I am "disoriented."

Even my appreciation of the three-dimensionality and configuration of any "solid object" before me reduces to a conglomeration of the directions and distances of all of the points which comprise its surfaces. To know that this is so, I have only to mark any two points on the object with dots of ink, and then take note that I see each of those points in a different direction from *myself*, and that one of the points is farther from *myself* than the other.

If visual orientation in space is to be improvable by training, the raw material must be the elements in the perception of direction and the elements in the perception of distance. I have already dealt at length elsewhere with the matter of direction (Walls, 1951a), and I am sure you all know that I am convinced that the direction in which a spatial point is seen, when it is imaged at a particular point on the

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†Member of faculty; Sc.D., Fellow, American Academy of Optometry.

retina with the eye in a particular posture in the orbit, is unalterable. When I say "unalterable," I mean *unalterable by training*. For of course the oculocentric and egocentric directions of points "alter themselves" in the phenomena of anomalous correspondence and of paralytic squint. There is nothing we can do that would bring on such alterations however—and precious little that we can do to make such alterations go away.

If only the deepest pessimism attaches to the trainability of the perception of direction, our fundamental optimism about visual training turns our attention to the elements in the perception of distance, or what is more commonly called "the perception of depth." Just what *are* these, and which of them might be "trainable"?

I make a sharp distinction between the kind of depth percept that depends upon retinal disparity and which is therefore possible only in binocular vision, and the host of other depth percepts which are just the same in monocular vision as in binocular. I deplore the use of the word "stereopsis" as relating only to binocular vision, and particularly the use of "stereopsis" as if it were synonymous with "stereoscopic visual acuity." Stereopsis means *solid-seeing*, and a one-eyed man sees just as solidly, just as three-dimensionally, in every day situations as does a two-eyed man. In the real world outside of the laboratory, the observer is never motionless for very long—and, the moment there is relative movement of an observer and the things he observes, the factor of *motion parallax* gives each of those things a position in space, and a configuration of shape, which are nowise dependent upon two-eyed observation or the observer's stereo acuity. Stereo acuity, indeed, plays an insignificant role in commonplace stereoptic vision. Any efforts to train *it*, in anyone excepting—say—a rangefinder operator, are philosophically wrong, and just as much a waste of time as it would be to train a high-jumper by having him jump over a pencil lying on the ground, practicing this until he could do it with his shoes just grazing the pencil.

I shall separate, then, the factors in stereopsis (= depth-perception) which do not depend upon binocularity from those that do. These latter I call *stereoscopic* factors, for they are what make the vision of a picture in a stereoscope different from the vision of the same picture outside of the instrument. Of paramount importance here is the existence of *horizontal retinal disparity*. The non-stereoscopic factors in depth are all available in monocular vision; but it is quite improper to call them "monocular cues," for they are equally available and valuable in binocular vision. It is also outmoded to call them "secondary

cues to depth" as many textbooks do. The distinction between primary and secondary cues used to be made on the basis of whether the cue was one which a realistic artist could incorporate into a painting (= "secondary"), or was one which he was helpless to imitate (= "primary"). There is one item on the short list of primary cues which we know to be no cue to depth or distance at all: this is *accommodation*. There is another "primary" cue, which is one of the stereoscopic ones: the extent of *convergence* required for haplopia (singleness) of a binocular point. This is of very little value as an informant of the distance of the point, and is certainly of no value at all beyond very close range. On the other hand, in our times the boundaries between hand-made art and camera-made art have broken down completely, and there is now a type of picture (the Winnek "Trivision" photograph) in which *relief* can be seen, so that by moving his head sideways the observer can even look *around* a foreground object and see another object which had been concealed from him when his head was in its original position.

Chief of all the non-stereoscopic factors—for a motionless observer and a motionless object—is *subtense*, i.e., the amount, or solid angle, of visual field the object appears to occupy. There is no doubt whatever that the use of this cue has to be learned, for the simple reason that the size of the retinal image of an object is completely ambiguous. It cannot connote the size of the object unless the distance of the object is known or thought to be known. On the other hand, it cannot connote the distance unless the observer has some idea of the object's true size. A baby reaches for the moon as readily as for a pingpong ball. A toddler has to learn to tell a distant transport plane from a sparrow close overhead. So far as the dependability of the subtense cue is concerned, a child can become accurate in judging distance only as rapidly as he learns the true sizes of the things in his environment. Any false perception of size inevitably causes a false perception of distance, and *vice versa*. Judgment of the size of an object may remain accurate until the object has been carried to a considerable distance, beyond which the judgment becomes more or less inaccurate. We refer to this inaccuracy as showing the quality of the particular individual's "size-constancy phenomenon." Another way of describing exactly the same *underlying* phenomenon is to plot, for the individual, a curve of the apparent distance of an object against its real distance. The curve is a 45° straight line for quite a span, but eventually bends over to a plateau. It should be obvious that if the estimates of large distances did not become incorrect there would be no breakdown of the judgment of size —or, if size-estimation did not become inaccurate, the accuracy of dis-

tance judgment would hold up. Which is it? Well, since it is the true distance that is being changed and not the true size, it seems clear to me that instead of worrying about why size constancy breaks down, we should have our attention upon the breakdown of long-distance judgment that causes this.

Another potent "depth" cue is *geometrical perspective*, the tapering of the retinal image of an object from its near end to its far end. For a row of houses extending down the street, or for the width of the street itself, perspective is nothing but the subtense cue occurring again and again. The retinal image of the street tapers. The perceived street does not. It *may*, for a very young child. But the child soon learns that things that look that way really turn out to have constant width when one moves along their depth-extent. We must be careful of this word "learn," however. Actually, all the child has to do is to "catch on" to the fact that man-made streets and buildings and tables and books all have parallel sides and square corners, regardless of their shapes in the retinal image. He learns this, yes—but this does not for a moment mean that we could expect to take an adult in hand, and give him any amount of training or practice in the employment of the subtense cue, with any expectation that we would *improve* his ability to appreciate squareness and parallelness. The kind of learning that involves "catching on," "getting the idea," cannot be continued indefinitely with continuing improvement of performance. Once a child has caught on to how to keep a bicycle upright, he can never keep a bicycle any *more* upright than that no matter how long he rides one.

Overlap is another cue to the relative distance of objects, the use of which has to be learned, but which it would be hopeless to expect to improve by practice or "training." If object *A* hides a portion of object *B* from me, *A* must be nearer than *B*, for it lies between me and *B*. I cannot remember learning this, over a long period of painful practice. Can you? Here again, once the individual has "gotten the idea" of what overlaps in the visual field signify, his whole training and his whole period of trainability in this respect are over.

Motion parallax is a distance cue of inestimable importance. The viewpoints of the two eyes in a motionless head are only two and a half inches apart. Even a one-eyed man has only to move his head to one side a few inches to be as well able to judge which of two objects is the nearer as is a two-eyed man standing still. One-eyed men do not bump into furniture and they do not have more than their share of traffic accidents. As we shall see later, one cannot even use his ordinary binocular stereo acuity at all to make the judgments of *large* relative

distance for which motion parallax is such a dependable cue. The learnedness of motion parallax has never been established, but it is safe enough to assume it. What teaches its use to a child, however, is probably another of the distance cues themselves—*i.e.*, overlap; for, as we move about, one thing temporarily occludes another and tells us that the occluded object is the farther one. I strongly question whether the ability to make use of the relative movements of near and far objects can be improved by any sort of training. An individual who had never caught on to motion parallax would be too stupid to be taught much of anything.

A neglected—that is, an often overlooked—cue to distance is the *elevation* of the object, particularly the base of the object, toward the horizon. One can localize the trees in a field by looking only at where each intersects the ground, for one must look downward in the case of a nearby tree, and lift the line of gaze higher and higher for more distant ones. Few people ever realize how much use they make of this cue—but, ask any football lineman!

Aerial perspective and illumination perspective, the blurring and "fuzzing" of very distant objects and the appreciation of "modeling" through the distribution of light and shadows, are cues to distance and three-dimensionality which can stand a great deal of training. One's education in the interpretation of unfamiliar shapes through illumination perspective never ends, and I do not doubt that some persons are better oriented in their environments, and others less well oriented, depending upon individual sophistication in this regard. But I know of no serious efforts to *train* the capacity, outside of art schools.

The whole outlook for any new rationale of visual training in space-perception, through increased attention to the non-stereoscopic factors, thus holds little or no promise. People do not in fact get into the kind of visual difficulties which training might help them out of, *except* in relation to binocular vision and their success or failure in using the special orientative cues it provides. We turn, then, to consider the *stereoscopic* cues to space.

There are three of these, that have ever even been thought to have any value. One—convergence—I have already dismissed. The other two are retinal disparity and physiological diplopia; but only the first of these is actually usable. The kind of retinal disparity that is meant is a small difference, from one retinal image to the other, in the apparent lateral separation of two points or two objects. It is a difference between homologous horizontal dimensions in the left-eyed and right-eyed images. If the points *A* and *B* (Figure 1) are at unequal dis-

tances, the apparent lateral separation of *A* and *B* will be greater for the eye on the side of the farther point.

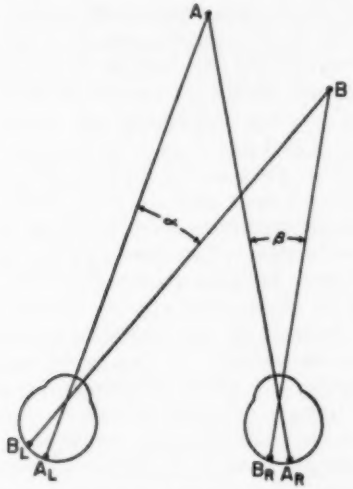

Fig. 1. The basis of retinal image disparity, in the difference of apparent lateral separation of points separated in depth, as seen from the positions of the separate eyes.

In the retinal images themselves, the points will be separated by different distances *d'* and *d*, and in the "binocular image" the discrepancy or difference between *d'* and *d*, or simply "*d'* minus *d*", is the retinal disparity (Figure 2).

We do not appreciate "d' —d" directly *unless it is larger than a certain*

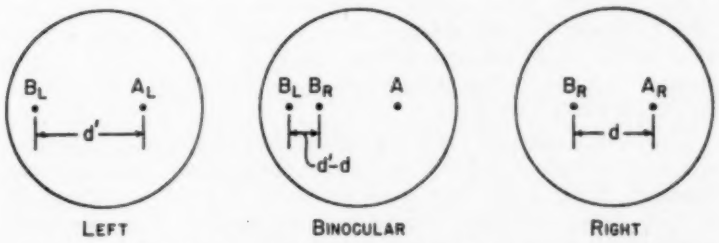

Fig. 2. The retinal images in the situation shown in Figure 1. Their horizontal disparity is *d'* —*d*. When this is small enough, *A* and *B* are both haplopic when either is fixated, and *d'* —*d* is "converted" to stereoscopic depth. When the disparity is too great there is diplopia of *B* when *A* is fixated, and vice versa.

value. When this is the case, we find that if we fixate *B* we see *A* "double," in *physiological diplopia*. Or, if we fixate *A*, then we see *B* double. When the retinal disparity is *smaller* than this "certain value," both *A* and *B* are seen singly, haplopically, when either one is fixated. It is particularly to this certain value—the amount of disparity at the borderline between haplopia and diplopia—that I will be calling your strongest attention in a little while.

Suppose, now, that we have started out with a pretty large difference in the distance of *A* and *B* from the observer. If he fixates one point, he inevitably has physiological diplopia of the other. Now, we reduce the difference in distance until "d′—d" reaches the critical value for haplopia to become possible for both points at once. Immediately, the observer is directly aware of *stereoscopic depth* between *A* and *B*. He needs no *other* cue to tell him which point is the nearer and which the farther; but if he closes either eye, the stereoscopic-depth percept vanishes. Retinal disparity of this and smaller magnitudes is, then, a potent cue to relative distance. In fact, when we say that "retinal disparity is a depth cue" it is tacitly understood that we mean disparity too small to create diplopia. One cannot begin to "use" retinal disparity until it has been made small, for large disparity means physiological diplopia—which can add nothing to our knowledge of the location of the diplopicized point. Note that no one with a normal visual system ever has to *learn* to use the retinal disparity cue to depth and solidity.

Now, let us reduce the depth-difference between *A* and *B* still further, and keep on reducing it. The value of *d′* will shrink toward equality with *d*, and the retinal disparity *d′*—*d* will get smaller and smaller. Eventually it will reach a value, below which the observer will *not* be able to say whether *B* is nearer than *A* or farther than *A*. For this small but finite value of *d′*—*d* there is a familiar name: *the stereo threshold*. This is usually expressed as an angle, in terms of α and β (Figure 1)—the difference between these angles being strictly proportional to the difference between *d′* and *d*. The reciprocal of this minimal value of α minus β is the measure of stereoscopic visual acuity, or "stereo acuity" for short.

The smallest value of α—β that enables a percept of a depth-difference between *A* and *B* is the only "stereo threshold" you have ever heard of. I call it the *lower* stereo threshold, however, to distinguish it from another one which I insist upon recognizing. This, the *upper* stereo threshold, is that "certain value" of α—β (or *d′*—*d*) which cannot be exceeded without physiological diplopia. There is, then, a *range* of values of α—β within which one experiences stereoscopic depth

between *A* and *B*. This range has never had a name except such a clumsy one as "region of binocular single vision." I shall call it the *haplopic range*.

Now it should be evident that the size of the haplopic range is bound to show variation from individual to individual, for each of its limits shows such variation. Smith, then, may have a greater range than Jones. Is Smith, therefore, better off than Jones? Most decidedly he is, and for two reasons, one of which is sensory or perceptual and the other of which could be called motor. If a greater haplopic range is a desirable thing, may it be possible to *increase* the range in an individual by some sort of training? I believe so; and it is the consideration of just this possibility and its instrumental exploitation that I want to urge upon you as my contribution to the results of this workshop. For a full understanding of the matter, we have now to consider the horopter.

If a practical clinician wants to frighten himself thoroughly, he has only to read the discussion of the horopter in a textbook, or in a reference work such as Ogle's (1950) "Researches in Binocular Vision." He will be impressed; but, he will not get an impression that the horopter is anything more than a mathematical toy of the researchers in physiological optics. No writer ever tries to take the horror out of the horopter, or tries to make it seem to have the slightest *practical* significance. I shall attempt both of those feats:

Ogle (1950) has studied the horopter and all its works for more than 20 years, so he ought to know how to define it. He states that: "In a general way the horopter would be a surface of some kind in space that passes ideally through the fixation point, and any other point of which has images in the two eyes which fall on corresponding retinal points, and which therefore will give rise to one and the same primary sense of direction." This does not help much until we know what he means by "primary sense of direction" and "corresponding points." He never tells us clearly.

Ogle does define a "principal visual direction." There is one of these for each eye, given by stimulation of the fovea (meaning, the very centralmost point in the fovea), and it is supposed to be the subjective direction of the fixation point. This lets eye movement come into the situation whereas it should have been kept out. Ogle then goes on to speak of "primary subjective directions" without saying where these come from, and defines corresponding points as *retinal elements* "which give rise to the same primary subjective direction." Perhaps "primary subjective direction" and "primary sense of direction" are supposed to

be synonymous. Perhaps the "principal visual direction" is one of the "primary subjective directions"—a particular, special one attached to the centralmost foveal element. But we are not told.

If this is the best that Ogle (leaning on Hering) can do, then I feel perfectly at liberty to erect my own definitions. I have already done this elsewhere (Walls, 1951a, 1951b). When a spatial point is seen haplopically, it has a direction from "the self," an *egocentric direction*. The "self," here, is the visual self, and the visual self sees all things as if it were located at a single point. This point is the perspective center of the dominant eye—the center of the entrance pupil. When "I" look at an object and take note of its direction from me, "I" am at that point and the direction of the object is a direction *from* that point. Even if I close the dominant eye, this does not change the situation. The object does not have two egocentric directions, one with respect to each eye, which are somehow "the same." It has only *one* direction, which is not "the same as" anything.

One element in the determination of egocentric direction is the pattern of innervation sent to the oculorotatory muscles. If the eye *turns* to fixate one point after another, the egocentric direction of each of these points is different. Yet each of the points is being imaged at the same place in the retina, the center of the fovea. Suppose, now, we go into the retina of the dominant eye and find a receptor which, when stimulated, gives us a subjective point exactly 10° to the left of the fixation point. No matter how the eye turns in the horizontal plane, the egocentric directions given by this receptor and the fixating one will always be 10° apart. There are, then, directions *relative to each other* which are "attached to" retinal points and which remain fixed relative to each other despite eye movements. These are *oculocentric* directions. Each oculocentric direction becomes an egocentric direction, for any posture of the eyeball, when the record of the innervation sent to the muscles is integrated with the retinal position of the receptor with respect to the center of the fovea as an "origin" or polar "zero."

Now for any retinal element in the dominant eye, which gives a particular oculocentric direction—say, "10° to the left of fixation"—there is an element in the non-dominant eye which gives a particular oculocentric direction. I shall not call this the *same* oculocentric direction, for we are not talking about the same eye, and each eye has its own set of oculocentric directions. This particular oculocentric direction in the other eye is, however, a homologous one—that is, it also is "10° to the left of fixation," for that other eye.

We know that when one spatial point is imaged at the center of

the fovea—"bifixated"—we see only *one of* that point: we see it haplopically. The oculocentric directions of the foveocentral elements are homologous. Any point seen in homologous oculocentric directions is seen haplopically, singly. The *egocentric* direction (Ogle's "primary sense of direction") has nothing whatever to do with the matter. The postures of the eyeballs have nothing to do with it. If my eyeballs were hauled out of the orbits and turned back to back, and the "10° leftward" receptor in each retina were now stimulated by a point source of light, I would perceive one source, haplopically.

In *my* glossary, "corresponding retinal points" are points which give homologous oculocentric directions. From here on, then, we can forget about *egocentric* directions, for these are determined by eye-muscle innervations and those innervations have nothing to do with determining oculocentric directions. The essence of a pair of corresponding retinal points is that they afford a haplopic percept of any spatial point imaged on both of them. The *reason* why they do has to be sought in the inborn structure of the brain: both members of a pair of corresponding retinal points are represented at *one* point in the visual cortex. We can in fact go directly to the cortex, in a patient undergoing brain surgery under local anaesthesia of the scalp, and stimulate one point there with a small electrode. The patient thereupon "sees," binocularly and haplopically, a "star" out in visual space (and in a particular *egocentric* direction which should depend entirely upon what his eye muscles happen to be doing at the moment.)

Back, now, to the horopter. Ogle discusses the points in space, each of which (for a given bifixation point) has its retinal images on corresponding points. He then says: "The sum total or the loci of all those points which satisfy this condition are said to lie on the *horopter*." This ought to make the horopter clear, but unfortunately it is the most ungrammatical sentence in Ogle's excellent book. What he seems to mean is that the part of external space in which lie all the points, imaged upon corresponding retinal points, *is* the horopter. A part of space, mind. It may be a *surface*—or, it may be found to be a *solid*.

One "determines" or maps the horopter with very simple apparatus (Figure 3). It provides a fixation point F (actually, a vertical rod), and a number of tracks all converged toward the interpupillary point. On each track a vertical rod can be moved toward or away from the observer to determine a point on the longitudinal horopter. Let us consider just one of these tracks, and upon it the point A (Figure 3), which may be moved to the positions A', A'', etc., while the observer maintains bifixation on F.

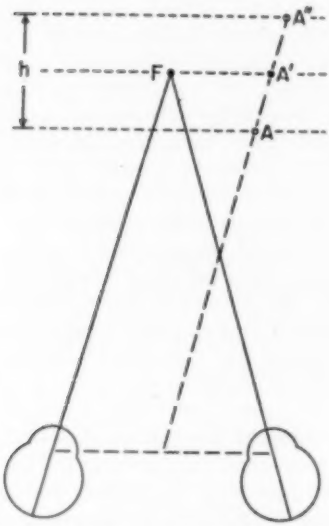

Fig. 3. Method of determining the haplopia horopter. With *F* bifixated, and *A* seen singly in any of the positions shown, *h* is the thickness of the solid horopter at the eccentricity of *A*.

How, now, are we to know when each movable rod, such as *A*, is on or in the horopter? Ogle lists six different criteria, which he has rounded up from the literature, each of which actually involves a separate and distinct definition of "corresponding retinal points." A spatial point may be considered to lie in the horopter when:

(1) "The primary subjective visual directions of its monocular images are identical."
(2) The point appears to lie in the same frontoparallel plane as the bifixation point.
(3) The point, and the bifixation point, appear equidistant.
(4) The point lies in the "center" of the "region of binocular single vision."
(5) The point's position is such that stereoscopic sensitivity to a toward-or-away change of position is at a maximum.
(6) The point does not stimulate fusional eye-movements.

Are these six different "horopters"? Horopter investigators think not—there is only one horopter (for a given bifixation point), and here are only six different ways of determining *the* horopter. But the

last of these ways is so far only theoretical and cannot actually be put into practical application. The methods which *are* practical do not all give the same result—and in the first of them, the method can work only if binocular vision of the movable rod is *prevented*, and yet the horopter is defined in terms of binocular vision!

Let me paraphrase the wordings of the workable criteria, and characterize the resulting "horopters" by simple phrases for the purposes of discussion. The horopter may be the locus of:

(1) Points, each of which has homologous oculocentric directions.
(2) Points, which appear to determine a frontal plane.
(3) Points, which appear equidistant.
(4) Points, each of which is "half-way" through the haplopic range.
(5) Points, each of which, for its eccentricity from fixation, is at a locus of maximal stereo acuity.

Ogle goes on to say immediately that his own definition of corresponding points allows only "1" to be a correct horoptral criterion. But the position of *A* (Figure 3) that gives it homologous oculocentric directions can only be determined by concealing part of the rod from one eye and another part of the rod from the other eye. The visible parts of the rod are then only seen vertically aligned, nonius-fashion, when its position on its track is such that its oculocentric directions are homologous. There is no point being seen in *binocular* vision (excepting the bifixation point itself), so that the "horopter" thus determined is a little unrealistic to say the least.

Even so, this "nonius" horopter and the frontal-plane horopter and the equidistance horopter all agree pretty well, except that the precision of nonius settings is said to be only one-third as good as the precision of frontal-plane settings. Horopter investigators, indeed, appear to make their choice of a criterion solely upon the basis of the closeness with which the observer repeats his settings. The underlying philosophy here is that the horopter "ought to" turn out to be a *surface*, and if the observer does not set each rod always at *the same* point (helping to determine a geometrical surface), then he is making *errors* in his settings. The criterion which allows him to make the smallest "errors" is adjudged to be the best criterion. We shall return to this conception—and to the unwisdom of defining the horopter as a surface before determining whether it is a surface or not.

Let us first, however, consider this matter of the three favorite criteria all giving "the same" horopter (when the observer's *average* settings are plotted). The work is actually done in only a very narrow

angular part of the visual field. The narrower it is, the closer the criteria agree—naturally. But in reducing the angle of visual field he covers sufficiently to get close agreement, the investigator is really reducing *himself* to the absurd. The horopter is not defined as having ony lateral *limits*. If I place a rod at 45° to the left of fixation and adjust it for equidistance with "*F*," I am certainly not using my stereo acuity to make the setting. I am merely comparing two distances and trying to equate them. My ability to do this does not depend upon imagery on corresponding points, and has nothing to do with the determination of a horopter. Similarly, if I adjust the 45° rod into a plane with "*F*" apparently frontal, this performance is again purely a *distance* judgment, and it has nothing to do with horopters and corresponding points. I can do the job as well with my right eye closed!

The first criterion, then, is non-binocular and the second and third have no relation to correspondency when eccentricity is not limited. Skipping the fourth temporarily, let us look at the fifth. It says that an eccentric point is in the horopter if a *small* depth movement of the point is detectible, and it is not in the horopter if the depth movement has to be *larger* than this to be detectible. This criterion is a cute trick for forcing the calculated horopter to turn out to be "surface." It is a purely theoretical criterion, and it does not work out in practice. Miles (1949) studied the problem of whether "physiological diplopia" really *is* a cue to depth—*i.e.*, whether one can tell whether object *A* is nearer or farther than object *B* when *B* is bifixated and *A* is outside the haplopic range and is diplopic. He found it to be no cue.* But the finding of Miles' which is of interest for us here is that as a point was moved through the haplopic range the position at which it was most often correctly judged to be nearer than the fixation point, and that where it was best seen to be farther, were far from coincidence. Instead of there being one point in the haplopic range (on "the horopter") at which stereo acuity was maximal, there were two, and these "straddled" the theoretical horoptral surface that would be determined by any other criterion.

This seems to leave the fourth criterion as the only remaining hope of putting common sense into the horopter and perhaps getting something practical out of it. To use this criterion, the observer draws the movable rod toward him (Figure 3) until it ceases to appear single

*Physiological diplopias in the visual field do however enormously augment *depth impression*, *i.e.*, one's conviction that one is viewing a three-dimensional scene. This is a purely qualitative effect, however, and one cannot *use* the amount of a diplopia or its direction (crossed *vs.* uncrossed) to adjudge the distance of an object in the absence of other cues.

(at *A*). He then pushes it away until again it ceases to appear single (at *A″*). Since the rod's track is not aimed at either eye, the image of the rod will move through an angle on each retina. There will be a point on the track—A′—corresponding to a bisector of that angle. This point is taken to lie on the true horopter.

When this has been done on each of many tracks, all of the points "*A′*" determine the horizontal intersect of a vertical *surface*. These points, remember, are not observed but calculated points, with absolutely no physiological significance to go with their mathematical uniqueness. The surface they determine is a horopter *only* if the horopter is defined in advance *as* a surface. Ogle does, indeed, so define it when he says: "In a general way the horopter would be a surface of some kind in space (etc.)." But if the horopter is also defined as the locus of spatial points which stimulate corresponding points, and if corresponding points give subjective haplopic points when stimulated, then *any* point from *A* to *A″* (Figure 3) is as much "on the horopter" as is *A′*. The common-sense horopter is not, then, a surface at all but a *solid*. Its thickness at the eccentricity of *A* is the distance *A*—*A″*. This is the thickness of what others call the "region of binocular single vision," or what I have called the haplopic range.

To understand why the real, common-sense horopter is a solid and not a surface, with a biological meaning and not an abstract mathematical one, we need one more diagram and one more concept. Figure 4 is like Figure 3 excepting that the fixation point has been placed opposite one eye (the dominant) and the track along which the *A* rod moves is also aimed at the perspective center of that eye. When *F* is bifixated and *A* now travels from the position of near-doubling *(A)* to the position of far-doubling *(A″)*, the image of *A* on one retina does not move, while the image on the other retina does move, through the angle *P*. Corresponding to *P* there is a *linear* distance on the retina, labeled "P.A.D." Since one image of *A* can lie anywhere within *P.A.D.* and still be combined haplopically with the *fixed* image of *A* on the other retina, it is clear that "corresponding points" are strictly a fiction. What corresponds, in one retina, to a point in the other, is a *spot* of considerable size. This spot is an *area of Panum*. The horizontal diameter of the area is *"P.A.D."*, and it is clear that *P.A.D.* measures, *determines*, the thickness of the horoptral solid at the locus of *F* in the visual field.

But, when the haplopia criterion is used for the experimental determination of the empirical longitudinal horopter, the typical investigator acts as if he had never heard of Panum's areas, and he calculates a *surface*, treating the radii of the Panum areas as *errors of setting* on

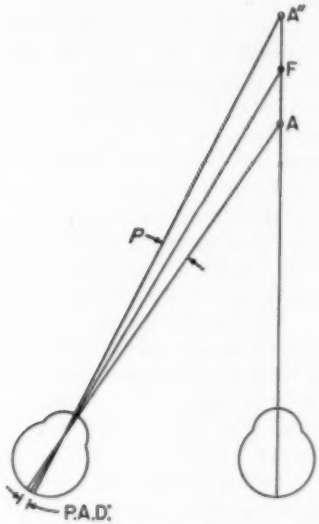

Fig. 4. Measurement of a Panum's area. With *F* bifixated, and *A* seen singly throughout the range of movement shown, the image of *A* moves on only one retina, through an angle *P* and a linear distance P.A.D.—the horizontal diameter of a foveal Panum's area.

the part of the observer. He can show that the sizes of these "errors" can be influenced: for, obviously, if a fatter rod is used it will have to be moved farther toward and farther away from the eyes before it can be seen diplopically.

Is it at all proper to dismiss Panum's areas as illusory, as a measure of *errors*, as a flaw in the design of the visual system? A million times, no! If Panum's areas were not "intentional" in the design, if they were not biologically *real*, consider what would happen:

When a bifixation point was adopted, the locus of all other points that could be seen singly *would* now be a geometrical surface, for correspondency would really be between literal points. How large an object could now be perceived as having stereoscopic solidity? No object larger than a point, for any point on the object that did not lie in the horopter would be seen doubly. Moreover, not even the bifixated point could be seen as a single point continuously. The "fusion" of the point would periodically break up; for, the oculorotatory muscles could not hold the eyeballs steady enough to maintain mathematical bifixation if the horopter were a mathematical surface.

The real horopter, the common-sense horopter, defers to these biological realities. Panum's areas exist in order that stereoscopically solid perception of solid objects shall be possible. Lacking such perception, there would have been no point whatever to the evolution of binocular vision in the first place. We sacrifice a great deal by having our eyes aimed forward, instead of having them on the sides of the head like a squirrel. We must gain more than we lose by this "move" of the eyes. But the *only* gain is *stereoscopic* perception—which we would not have, but for the Panum-area phenomenon.* And, Panum's areas are *equally* necessary to provide for a tolerance upon the precision with which the rotatory muscles must operate. Those muscles are not steel rods actuated by mechanism with its parts machined to a millionth of an inch. They are living tissue with all the physiological variability of living tissue, and are controlled by neural mechanisms which are also of living tissue with physiological variabilities of their own. In the evolution of the Primates, Panum's areas had to be "invented" just as soon as binocular vision was first attempted. They are immensely more ancient than the *human* visual system. They must have primary consideration in any definition of the horopter that can ever make any sense.†

Panum's areas, then, cause the horopter to be solid with a finite and valuable thickness. From the motor standpoint, this "takes the pressure off" the extra-ocular muscles and allows them to be reasonably inaccurate in their settings without this doing any harm. From the perceptual standpoint, the importance of the thickness of the common-sense horopter is that the entirety of any object which falls within it, or an equally thick "slice" of any larger object the "depth" of which we explore with moving fixation, is appreciated instantaneously in its stereoscopic solidity—even when the object is totally unfamiliar. No learning process is required for stereoscopic perception, which is why even the stupidest of our monkey-like ancestors (who invented binocular vision) found that they thereby gained more than they had sacrificed, in limitation of visual field.

It has taken all these words to reach the point at which I can explain what it is that I propose should be "trainable," and should be

*We do not need to go into the matter of the physiological basis of the phenomenon, here. Largely, it is a cortical one: but a "portion" of the Panum area at each retinal locus, antedating even binocular vision phylogenetically, is given by the "receptive field" of each optic-nerve fiber (see Walls, 1953).

†The dimensions of Panum areas which Ogle (1950) gives for different eccentricities (his Table 7) are quite useless. Some of them, including those quoted from Ogle's own 1932 paper, were calculated from data for the nonius horopter. The nonius method cannot give the size of a Panum's area, for *haplopia* is not even allowed to occur in that method.

attempted to be trained, for the improvement of the value of a patient's binocular vision as an informant of his orientation in his visual environment. You all know how much effort has been spent, and is still being expended, in attempts "to increase stereo acuity." These are attempts to reduce what I have called the *lower* stereo threshold. What I have called the *upper* stereo threshold, you can now see, is nothing but a measure of the size of a Panum's area in the neighborhood of the retinal fixing point. Now what I should like to see is an attempt to "train" an enlargement of Panum's areas.

Orthoptists spend much of their time trying to train motor fusion —with enough success to keep them optimistic about tackling the next case that comes in. They concern themselves somewhat also with training "sensory fusion," working doggedly with the patient until, in favorable cases, he suddenly achieves stereoscopic vision—just as one suddenly learns to keep a bicycle upright. Many people think that there is something more yet to be done. Some have the idea that by building up the stereo acuity they will thereby clinch the patient's possession of stereoscopic perception and insure him against losing it again.

To my mind, this is exactly the wrong way to try to "build up" stereoscopic visual capacity. One is only teaching the high-jumper to leap neatly over pencils. Of what importance is stereo acuity, anyway? One depends upon it to decide whether *A* is just as far as *B*, or a bit nearer or farther. Stereo acuity enables a judgment of equality, or inequality, or distance. But in using it, the visual system is serving only as a null instrument. Stereo acuity does not enable one to say *how much* nearer *A* is than *B*—for such an estimate, we are dependent upon other factors. Let the depth-separation of *A* and *B* become very much more than "relatively small," and physiological diplopia of one or the other supervenes. Thereafter, there *is* no binocular stereo acuity, and if any accurate estimates of relative distance are made, they are made on the basis of motion parallax.

On the other hand, if Panum's areas can be enlarged, the haplopic range is extended. The thickness of the solid horopter is increased. It is not very important, to a patient, how small a difference in distance he can detect. But it *is* important, how big a chunk of space he can perceive stereoscopically at a glance. It *is* important, how much leeway his eye-muscles have, in their positioning of his eyes. It *is* important, how much of the thickness of his horopter is used up by his heterophoria or his fixation disparity. So, it *is* important, how large his Panum's areas are, and important to know whether they might be made larger.

This would mean training and practice with a stereoscopic instru-

ment. But one would not be trying to see how small a retinal disparity the patient could "detect" through its conversion into an appreciable perceptual depth-displacement. Instead, one would be trying to see how *large* the retinal disparity could be made today as compared with the other day, and still have the patient capable of perceiving *B* haplopically when he was fixating *A*.* The patient who succeeded in materially extending his haplopic range—in "thickening" his common-sense horopter—would then be the one for whom the stereoscopic mode of perception would be most thoroughly clinched, and would be the patient least likely to be gotten into motor difficulties by his heterophorias.

Is there real hope that an attempt to "train" the upper stereo threshold to enlarge would succeed? There appears to be. It seems that nearly a hundred years ago (in 1859) Volkmann tried just this. At an eccentricity of 2° from the fovea, the horizontal diameter of his Panum area when he first measured it was 12.1 minutes of arc. Volkmann set about trying to reduce it. He succeeded, "after long practice" (Ogle, 1950: table 7, footnote), in getting it down to 5 minutes. He also practiced to try to *enlarge* the area, and was able to increase its horizontal dimension to 25 minutes. This represented an increase of more than 100 per cent. Such an increase seems a very fair return, indeed, for any visual training procedure to yield!

I urge, therefore, that it be seriously considered whether the visual-training armamentarium should not incorporate means of attempting to increase the haplopic range by moving the *upper* stereo threshold to higher and higher values. Train motor fusion, yes. Train everything trainable, by all means—perhaps even stereo acuity. But try also to train the patient to get the most good out of his horopter. For the horopter, the real, common-sense, *solid*, horopter is an intensely practical thing. It does not deserve to be buried forever under a bushel of words and symbols in the textbooks of physiological optics.

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*Note that *haplopically*, not merely *singly*, is stipulated. The patient's visual system may of course avoid diplopia by resorting to rivalry or suppression. Successful training of the haplopic range would be, concomitantly, a training of the system *not* to employ such dodges.

SUGGESTIONS FOR A NEW FEE STRUCTURE IN OPTOMETRY*

Carel C. Koch†
Minneapolis, Minnesota

PART III‡

This portion of the report deals with reactions to the proposals dealing with suggestions for a new fee structure. See outline on following page. The comments which follow are presented to give the opinions on the subject of men who have successfully conducted optometric practices for some time. These men have given the proposals presented in Part I considerable thought and study. None of these men was in the original group which first followed these proposals and experimented with them. Some have used portions of the proposed plan in their own practices and others have not. All have made significant contributions to optometric thought in the past and their views and suggestions on this question are of considerable value, presented as they are at this time when the entire question is being given much study by many optometrists.

CRITICISMS AND COMMENTS

Wick[1] in his communication first refers to the proposal that two separate bills be presented to the patient, the *first* for the examination and refraction fee and the dispensing fee, and the *second* bill for the cost of the materials at the prescription laboratory cost of these materials. Wick then suggests that perhaps it would be better to present two bills as suggested, but he believes the first bill should cover only the examination and refraction fee, and that the second bill should include the material at the prescription laboratory cost, plus the dispensing fee. He says, "This is a small point, perhaps, but it does help to keep it straight in the patient's mind that there is a separate fee for the examination and refraction, and a second fee for the materials, plus the dispensing of these materials. After that, whenever the patient gets additional materials, they expect to pay for these, plus this dispensing fee."

In addition Wick proposes that, "as long as we are trying to set

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†Optometrist, Editor, American Journal of Optometry and Archives of American Academy of Optometry. Fellow, American Academy of Optometry. Secretary, American Academy of Optometry.

‡Part IV of this report will appear in the October issue. Part I appeared in the July issue. Part II in the August issue.

OUTLINE OF SUGGESTED FEE STRUCTURE

The author has proposed a simplified fee system for optometrists in professional practice. A minimum examination and refraction fee of $10.00 or more is proposed. In cases requiring specialized or unusual care, fees larger than the minimum are suggested.

A minimum dispensing fee of $5.00 for each pair of glasses dispensed is also suggested.

It is further suggested that all ophthalmic materials be dispensed to the patient at the prescription laboratory cost.

It is further suggested that the patient be presented with an individual itemized statement for professional fees, i.e., examination and refraction fee, minimum of $10.00 or more; plus dispensing fee of $5.00, if materials are dispensed.

In addition, it is suggested that all laboratory materials be collected for in full at the time the material is ordered, as this material is being dispensed to the patient at cost, and the optometrist should not, and cannot, carry any of these costs or charges on his open accounts.

The reader is referred to Part I of this report for the details of this proposal. (July, 1952, issue.)

up a fee structure for the future. I have wondered for some time about the idea of charging in advance for subsequent adjustments of glasses which might never be used. I realize that some businesses do this in what is known as a *Service Fee*. I question if it has any place in a profession, but it has always been done that way in our work in the past and it is hard to break a tradition. Would it not be better to work towards the idea of always charging for adjustments on any and all glasses, whether it be for your own or other patients. In other words, a dispensing fee is all right but it should cover only the actual work done. Subsequent adjustments due to accidents or wear should be charged for as these arise. I have never heard of any guarantee for adjustments on materials being given for the lifetime of the item, but that is what we seem to do in optometry. Of course, we must realize that this sort of a change is a long way off, but is it not something to start working for now?"

In addition Wick states, "I cannot go along with the idea that the original dispensing fee (of $5.00) charged one's own patient should take care of the adjustments and service necessary when this patient has broken a lens. In these cases the case record has to be taken from the files and the decision made (by the optometrist) as to whether the lens should be replaced or whether a new examination and refraction should be made. If the lens is replaced the prescription must be written up for this, and when the laboratory has returned the glasses the lens must be neutralized to determine if it is properly made up. It is then necessary to realign the glasses to the patient's face, as most accidents will change the mounting sufficiently to require some adjustment. It would seem to me that the above professional judgment, office time involved, and

optometrist's time would necessitate a dispensing fee of at least $2.00 on single vision lenses and $3.00 on bifocal lenses." Perry,[4] Borish[5] and Andreae[6] all make much the same comment as is here brought out by Wick.

Wick's comments close with the following, "Take the above points for what they are worth. Personally, I think you have done a fine job in bringing up the suggestions for a new fee system in the Academy journal. This is another place where leadership in the Academy can be quite a value. I am looking forward to the other issues to help clarify my own thinking and feel certain your reports will result in some changes in my own fee system."

Springer[2] states, "I would like to thank Koch for bringing to the profession's attention the necessity of the individual optometrist carefully explaining to his patient exactly what he is paying for when receiving ophthalmic attention. Too few have done this, and as Koch states, much confusion and unjust thinking exists on the part of the public. The plan suggested may seem a radical departure upon first observation. Actually, the only real change is in the use of the prescription price list with the patient and the proposal that all replacements be made without a service fee of any kind.

"Practitioners who have explained their total fee system in the past have little adjustment to make under this plan. Their patients should already understand what this system is attempting to show. For the optometrist who has not explained his fee, the plan will help to justify in his own mind, as well as in that of the patient, the increase in the significance of the professional services and the decrease in the value attached to the materials.

"There is little question," claims Springer, "that the average patient is soon to know the intrinsic value of ophthalmic materials. The unrestricted sale of corrective materials by manufacturers at prescription price to industry, and the provision of such prescriptions at no cost to many government employees, has hastened a process which the evolution of the profession would have otherwise achieved at a more normal rate. This speed-up makes it imperative that optometrists who have failed to explain their fees to the patient consider at once some plan by which they can do so.

"While I am in agreement with the goals of this suggested fee schedule, there are several points which bear discussion," says Springer. "First among these is the breakdown of the professional fee into an examination and refraction fee and a dispensing fee. If this is done so that the dispensing of a pair of glasses increases the professional fee

by 50%, the stimulus to prescribing glasses 'needed or not' will still appear strong to the patient. The optometrist should charge for services rendered, skill involved and time consumed. If he provides more of any of these, his fee should be greater. In the optometric concept, dispensing of the prescription is generally considered a logical continuation of the professional act. Why should the fee be separate if the service is not?

"In the third part of the plan it is proposed that the ophthalmic materials be dispensed to the patient on an itemized prescription cost basis and billed separately. I can see nothing professional in this procedure. The optometrist actually describes himself as a purchasing agent for the patient. In the other professions," Springer claims, "the materials consumed such as x-ray plates, dentures, legal manuscripts and abstract papers, etc., are not itemized as to cost. The public considers these things as necessary tools, so to speak, for the professional man to work with.

"It is my opinion that such itemizing will not protect the public from the unscrupulous price advertiser but will tend to justify his false claims. If glasses are provided at or near the advertiser's price, the legitimate optometrist will appear to have been very dishonest in the past. Actually, this is a hypothetical problem as the glasses are never available for this figure, but some explanation would still be required. I feel strongly the need of explaining the true values involved in visual care. I believe this can be done, however, without resorting to the use of price tags and catalogues.

"One of the advantages of the new system is stated to be the elimination of the monthly laboratory bill and the placing of the optometrist's account on a discount basis. The bill will still be due and the optometrist must still control his spending in order to have sufficient funds to pay his bills. Under any system, unknown patients should be required to, and they expect to, pay a deposit. There is usually no reason for the optometrist to have a large supply of materials on hand and certainly none to use 'bargain' materials or cut price laboratories. Only practitioners who do not understand their own economics do this sort of thing.

"I think the most significant portion of this paper is to be found in the advantages to the patient. The patient should have these advantages. Actually, if the optometrist has explained his fee, the patient already has them. I do feel, however, that some fee over the prescription cost is justified in replacement situations.

"Many optometrists in this area," says Springer, "have provided

each patient with a printed form in explaining the economics of the fee. This form follows:

"YOU DID NOT BUY THESE GLASSES"

You have bought time, skill, knowledge, and service. You have bought the following:

1. EXAMINATION: Professional examination for possible pathological eye conditions.
2. REFRACTION: Scientific measure of your ability to see.
3. PRESCRIPTION: Carefully prepared professional conclusions and proper instructions necessary to correct your vision.
4. INTERPRETATION: Careful technical and scientific compounding of the exact materials of your prescription.
5. FITTING: Scientific, minute adjustment of your prescription to your eyes.
6. RE-EVALUATING: Verification of the refraction and the prescription.
7. SERVICING: Assurance that the requirements of your prescription are being constantly maintained.

Professional services are worth infinitely more than the cost of the material in the glasses themselves.

"With such an explanation most patients can grasp the idea. When the fee for auxiliary corrections and replacements is greatly below the original fee for the full service, the value of the examination and refraction is vividly demonstrated for the patient. This difference should be explained to each patient.

"I hope," says Springer, "that every practicing optometrist will give careful study to the plan suggested by Koch. Its application in the practitioner's own thinking or in actual practice will aid him in adjusting to a changing economic situation."

Lawrence A. Fitch,[3] Dean, Pennsylvania State College of Optometry, has the following comments and suggestions to make:

"Part I of 'Suggestions for a New Fee Structure' is excellent. You have presented your suggested fee schedule so clearly in this article that any optometrist with this article as a guide should have no difficulty in adopting it. I hope you will have reprints made so that I can purchase them for distribution to students in my classes on office practice this year and in following years.

"Basically, your suggested fee structure is the same as that taught at the Pennsylvania State College of Optometry for the past 10 years. Many of our graduates are using a fee structure which provides the ophthalmic materials at laboratory prescription cost, and those graduates can vouch for the desirability and practicability of this system of charging patients.

"In teaching the fee structure to undergraduate students, we are not so definite relative to what fees should be charged as you have been in your article. However, it is interesting to note that among those optometrists with whose practices I am familiar, the $10.00 examination fee and the $5.00 dispensing fee are most typical. Few of these

optometrists vary their fees because of prismatic or multifocal components in the patients' prescriptions. On the other hand, many optometrists charge separate additional fees where more extensive services are necessary, such as the plotting of visual fields, the teaching of the use of sub-normal vision aids, the fitting of contact lenses, the rendering of orthoptic treatment, etc.

"In teaching fee structure to our under-graduates, we present several other details in our plan which are either not mentioned in your article or else differ slightly from your presentation:

(1) We recommend that the routine examination and refraction be divided into two visits, as we feel that is easier on the patient and conducive of better results.

(2) We recommend appropriate fees for repairs and replacements. Fees of $2.00 or $3.00 are recommended to be charged in addition to the laboratory charges for materials for replacement of a broken lens, a broken frame, front, etc.

(3) We recommend that repairs and replacements for non-patients be discouraged. Doing repairs and replacements for non-patients is seldom appreciated by the public to the extent that it leads to new patients; rather, this practice leads to the development of a repair business and interferes with the growth in prestige of the young optometrist. Often the person who approaches the new optometrist for adjustment or repair of his glasses does so because of the convenience of the optometrist's office. Convenience for these services will encourage the person to continue with his former eye doctor for major eye care services.

"Under advantages to the optometrists, there is an important advantage which you haven't mentioned. Less of the practitioner's time will be consumed in making adjustments to old dilapidated frames as the public learns that new frames are relatively inexpensive. Also, since a patient will be more likely to have two pairs of glasses of the same prescription, or his immediate past glasses intact, the optometrist will be called upon less frequently for rush emergency repairs and replacements.

"Under advantages to the patient there should be mentioned the fact that the fee structure results in more effective eye care through the greater use of new materials. Certainly a good fitting bifocal prescription or a high cylindrical prescription in a new frame will provide a more effective correction than the same prescription in an old frame that is constantly out of alignment.

"In closing, I want to again commend you on the clarity and simplicity with which you have presented this subject."

Perry[4] presents these additional thoughts. "I think the report

covers the subject quite well and I realize that the possibility of socialized medicine in this country becoming a reality prompted its release at this time. In general, I believe the report will be interpreted as being revolutionary. Considering your work on the National Interprofessional Committee on Eye Care, one possibility for criticism that comes to my mind might arise from the dispensing optician. However, I think much good will evolve from a careful study and application of this plan, so far as local circumstances will permit without creating too drastic a change."

Borish[5] brings up a point which the writer is sure has occurred to some optometrists. Particularly those optometrists whose practices are conducted in a medium sized community where one or more very well known physicians also refracts and where their fees for this work are exceptionally low. This problem exists only occasionally, but where it does, the optometrists in the area are faced with a difficult problem.

Borish points out that in one certain area the local eye, ear, nose and throat men (there are several of them) refract for a fee of $7.00. They all dispense their own materials at the customary markup. The optometrists in the area follow the same practice and have largely the same schedule of fees and charges for materials.

Now, claims Borish, in the face of this established pattern of pricing it would require an enormous amount of self-confidence on the part of an optometrist to impose a drastic revision of fees on his patients in the face of the very low schedule of fees maintained by those well regarded physicians in this area where matters of this kind are common household knowledge. Borish wants to know how we can get these physicians to make the suggested switch in fees. Borish, too, like the others quoted, sees many advantages in the new proposed plan, but in an area such as is mentioned he thinks the optometrists are in a rather difficult position. In Part IV of this report this question of admittedly low fees will be further discussed by the writer.

Andreae[6] feels that most of the older and well established practitioners are now using fee systems which seem to meet the requirements of their practices and that these men will be more or less reluctant to revise these procedures in line with the newer suggestions. He also points out that the younger men in practice should have no difficulty in switching to the new suggested fee structure with certain minor modifications, and that he thinks they will do very well with it.

Let us now consider the reactions of Barstow.[7] While not himself in practice he has served for years as a business and professional counsel to optometry in general and particularly to those men enrolled in the

Optometric Extension Program. Barstow says: "First of all, let me congratulate you on your interest in the subject and the fact that you have given it space in the *American Journal of Optometry*. This publication should be a great support to the idea of professional fees rather than a price for glasses.

"You can be sure of my harmony with the general principle because, as you know, this is the plan that I have advocated not only in my book but as far back as 1935, when I first proposed the principle in my paper for the Optometric Extension Program.

"There is one factor in the entire plan that bothers me, and that is, your dispensing fee of $5.00. You are familiar, of course, with my discussions of overhead and chair cost which I have used in lieu of a dispensing fee, although I like the phrase "dispensing fee" very much. The situation, however, is this, that from my statistics as they are furnished me by various optometrists nowadays, the fixed charges (fixed expenses of chair cost, overhead or whatever you like to call it) are practically always greater than $5.00. With increased costs, I find that in some offices this runs as high as $9.00 per patient and quite frequently from $6.50 to $7.50.

"These fixed expenses must be recovered by the optometrist in his fee or else they come out of his personal income, and I can see no reason why his $10.00, or whatever-dollar examination and refraction fee, should be diluted by his paying any part of these fixed charges. It would appear in your plan that the uniform dispensing fee of $5.00 is designed to cover this chair cost but, unhappily, the amount does not do it at present costs of operation.

"You will note that I have indicated three items involved in the total fee: (1) the professional compensation; (2) the overhead and (3) the cost of the ophthalmic materials.

"As you are also aware, Glenn Moore in Chicago has been following your plan of separating the cost of the materials completely from his professional services, and for quite a while in my lectures I presented this method. In my book I offered a choice and did this because I found so many men—for reasons they considered valid (whether I did or not)—were unable to bring themselves to the itemized method and so, on the 'suffer it to be so now' basis, I let it ride."

The point raised by Barstow on the dispensing charge not fully covering the "chair-cost" will be discussed in Part IV of this report. Before we leave this point, however, let me say that it was not intended in the suggested new plan that it do this.

Barstow mentioned Dr. Glenn H. Moore, optometrist, of Chicago.

author of the monograph, *Building a Successful Professional Optometric Practice on Sound Economics.* He mentioned the fine work Moore has done in the past in leading the way in eliminating those fees now hidden in part of the markup on materials. The writer, too, is well aware of the excellent leadership given optometry by Moore in this and many other phases of our work and professional lives.

In reviewing Part I of this report, Moore[8] says: "I have read with great interest your article, 'Suggestions for New Fee Schedule.' Having long been an advocate of separation of fees for professional services and charges for material, I find it easy to concur, in general, with the suggestions made in the article.

"The June, 1947, issue of the *Journal of the American Optometric Association* carried an article that I had written, entitled, *'Building a Successful Professional Optometric Practice on Sound Economics.'* At the time that article was run, I had been using the system described, which coincides very closely with the one you suggest, for several years. Had I waited a half-year longer to have written the article, I would have recognized two faults in it, which I have long since corrected in my practice. I soon recognized the fallacy of adding an overhead percentage to the laboratory cost of the material and have for about five years supplied the material at exact cost, to the next highest even dollar. I also recognized the fallacy of a fee for professional services including a re-examination a year in advance. For the past five years, fees for professional service include all of the services rendered up to and including the one month progress report, but not beyond that time. Also, during this five year period, fees for professional service have been raised to just about twice what they were in the beginning.

"I doubt," says Moore, "that most optometrists will want to price mark a frame, even though it is the laboratory's price mark, or to have it come out in fractions of a dollar. I prefer to use the next highest even dollar. I see no objection to the use of two separate statements, although I use one. See Figure 1.

"I am sure that all who have read the article will understand that your suggestion for a minimum fee is indeed minimum. I doubt," claims Moore, "that the average optometrist can exist on such low fees, if he is to take the time to do the kind of job that progressive optometrists do nowadays.

"All in all, I agree with the philosophy of the article. The facts simply are that the public has not learned that optometry is a profession, like they have about medicine and dentistry, and it is necessary for us to do things that these older professions do not have to do

GLENN H. MOORE, O. D.
6226 NORTH BROADWAY
ROGERS PARK 4-7032
CHICAGO 40, ILLINOIS

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FOR PROFESSIONAL SERVICES RENDERED.....		
REIMBURSEMENT OF LABORATORY COST.....		
All ophthalmic materials (lenses, frames, mountings, etc.) are supplied at laboratory cost. The item **"Reimbursement of Laboratory Cost"** represents the actual expense incurred in having your prescription filled and supplying your glasses. It is charged as a reimbursement of such incurred expense.		

RECEIVED PAYMENT_____

_____

Fig. 1.

in order to get the point across that we do not derive our income from glasses.

"I know of a fairly large number of optometrists who have been doing it this way for a number of years and I am sure none will ever go back to the old method of one unit fee. I assure you that I feel even stronger on the matter today than I did five years ago."

Hirsch[9] points out that the questions which are raised in this report are very important to optometry. He further believes that Academy members should interest themselves in arriving at the proper solutions, particularly with the aid of the A. O. A. Hirsch questions the propriety of a scientific body entering this field. He also regrets that a frank discussion of the question is appearing in the Academy journal, believing that the problem should be solved within optometry. Hirsch

points out that this publication is widely read outside of optometry and that it has considerable scientific status.

Mote[10] on the other hand says, "your discussion of the problems involved in practicing on a professional fee basis shows good understanding of the problem. I think that it is generally agreed that the whole problem of professionalism offers far more difficult solution in the large cities than in the small cities. It could be fairly accurately estimated that the magnitude of commercialism is about inversely proproportional to the size of the city.

"We must also keep in mind what Heather has said many times in his lectures on fees, 'It is difficult to correct 50 years of faulty thinking.' In many respects I think a remarkable job has been done in a very short time in teaching optometrists to think of fees in terms of what they do rather than by a markup on ophthalmic material prescribed. I am rather optimistic that the problem is nearer solution each year through converts to it.

"I cannot fully agree with your analysis of the distribution of the total fee between the examination and the design and fitting of spectacles. While it is true that the design and fitting operation may be delegated to an assistant, it is still the basic responsibility of the optometrist. It is not in the best interest of the public for him to delete basic supervision and responsibility for this act. I think that we will agree that these operations are becoming more difficult and time consuming each year. On a time consumed basis if the job is well done and considering interim visits for adjustments and incidental services, the time consumed will equal if not exceed the time consumed for the examination and refraction. Therefore the fee for the many services involved in the design, ordering the material, checking and verifying it, the fitting and the visits for adjustments should approximately equal the fee for the examination and refraction.

"In my opinion in order to be consistent, there must be a considerable latitude in what you describe as the examination fee. First there must be a fee for what I term an office call for optometric advice. Many mothers will want to bring children for visual clearance before starting to school. In many cases this will involve about one-half the time consumed in a complete examination and refraction in order to tell the mother that their vision is satisfactory without further care at present.

"I am enclosing for your files the administrative procedure and fee schedule used by Ohio optometrists in handling cases for the State of Ohio Bureau of Aid for the Aged. This procedure was drawn following a law enacted by the Ohio Legislature in which Optometric

services were mentioned specifically for the recipients of aid under certain conditions. One very interesting angle about it is that the cost of ophthalmic material is charged at laboratory cost as you are recommending.

"I cannot agree with Hirsch that this subject does not warrant discussion in the *American Journal of Optometry*. I have always felt that the Academy should be an organization that contributes to the professional, economic and social development of all optometry as well as the front line contributor in the scientific and technical development of our profession."

CONCLUSION

Comments on the suggested fee structure as presented in Part III of this report indicate an overall agreement with the general underlying principles of the suggested fee structure. These comments also present variations in techniques which are both of interest and value. The author is grateful for these criticisms and suggestions presented by his colleagues. These men, all leading members of the profession, have made contributions which will be of value to all optometrists who are studying the question of professional fees at this time. The writer also wishes to thank the great many other optometrists and readers who have expressed their views and letters of encouragment.

A summation of all the ideas, suggestions and criticisms received has been presented in Part III. In Part IV, these will be discussed by the author. In Part V, the author will review the timeliness of this proposal in relation to the political, economic and sociological aspects of the problem. Part IV will appear in the October issue.

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Publication and Academy Office: 1502 Foshay Tower, 821 Marquette Ave.
Minneapolis 2, Minnesota

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OPTOMETRY WELCOMES SHEARD AT LOS ANGELES COLLEGE

The announcement has come that Dr. Charles Sheard has assumed the position of Dean at the Los Angeles College of Optometry. He succeeds Dean Henry Hofstetter now at Indiana University.

We are not too surprised at this turn of events, but we *are* highly pleased, as we have felt for some time that Sheard would like to return to optometry where he first achieved nation-wide prominence as an educator and scientist.

Sheard, born in 1883, was, in 1914, the first director of applied optics at the Ohio State University where he had taught physics since 1907. Under his excellent start, this new course became in time the School of Optometry. In 1919 Sheard left the Ohio State University

to direct the division of ocular interests at the American Optical Company where he stayed until 1924. That year he moved to Rochester, Minnesota, where he became director of the division of physics and biophysical research of the Mayo Clinic, and professor of biophysics under the Mayo Foundation, University of Minnesota. He retained these posts with honors until he reached the retirement age in 1949.

Since 1947 Sheard has also taught ophthalmic optics at Rochester (Minn.) Junior College in the first two-year college course ever set up in this country for training opticians. Sheard has held this post until the present time. He played an important role in developing this educational activity.

Since 1949 Sheard has also been a distinguished lecturer in the department of ophthalmology, graduate school of medicine, Tulane University, during each winter quarter.

From 1924, because of pressure of other work, Sheard had less and less time to devote to his many friends in optometry, but his first scientific interests were always in the field of vision, and he never lost his early enthusiasm for optometry. Whenever possible he was back with his optometric friends, and we, in the Academy, well remember his many pleasant visits with us and the sound basic advice he always gave us. Sheard has ever opposed the "isms" and pseudo-science recommended by a few in optometry, and he could be ruthless if the occasion required; but the older generation of today's practitioners who knew him prior to 1924 still regard him with great affection.

A whole new generation of optometrists are now in practice to whom the name Sheard is just a legend. They have never met this pleasant, kindly gentleman with the long white hair—they have never experienced the warmth of his friendship—nor have they had the opportunity to be taught by this world renowned scientist and teacher. Now that he is back with optometry we hope he has a chance to visit around and get acquainted again with old and new friends.

Sheard did much to help optometry in its early formative years, and we in optometry today are truly grateful to him for his help.

His new job as Dean at the Los Angeles College of Optometry will be a stimulating one for him. And also, a new experience. In his long educational career this is his first assignment in a college not at present affiliated with a university. And here, too, he will make his contribution to all things which go into the making of good optometrists.

Welcome back Charles! Optometry everywhere honors you and wishes you well in your new position.

CAREL C. KOCH.

MANY METROPOLITAN AREAS NEED OPTOMETRISTS

During the past several decades, there has occurred a marked change in the geographic distribution of optometrists in practice. In 1910, when optometry was in the midst of securing state regulatory legislation, the majority of optometrists practiced in the larger urban centers. Gradually, newer graduates established practices in smaller communities, and today optometrists are found practicing in nearly every community regardless of size. This is one of the great strengths of optometry, and it is a factor which must be maintained to enable optometrists to best serve the public.

This movement, in which many optometrists established their practices in smaller communities, was due to many factors, one of which was the concerted drive by educators and other optometric leaders who pointed out to new graduates the advisability and advantages to him in taking this step. That this advice was followed and found good is now evidenced by the many very successful optometric practices which have been established in the smaller towns with their excellent trading areas.

Optometrists are today to be found virtually everywhere. A review of their present practice locations indicates that the trend is continuing. In the nearly 30 years this publication has been serving optometry, our circulation lists show this trend. At one time, most of our readers practiced in cities with populations of 35,000 or more. Today nearly one-half of our readers practice in communities of 20,000 or less.

This well developed expansion of optometric service into every rural community and into every small urban center is indicative of the fine service optometry is providing—a service which must be continued in the future.

There is, on the other hand, an aspect of this development which should be given some thought. As more and more young optometrists established their practices in the smaller centers, fewer and fewer new men came into the large cities to practice. While this is not true of every major urban center, it certainly is true of quite a few. In these latter cities, we find the number of optometrists in practice at the present time is little or no greater than the number who were in practice in 1925. These cities have all grown substantially during this period, but the optometric population has failed to keep pace with the growth of the community.

This, too, is a unique situation which certainly needs correction wherever it occurs. In these larger centers, opportunity also exists for

the establishment of excellent optometric practices—not only in the loop areas of these cities, but in the outlying shopping centers. The metropolitan public deserves good eye care and appreciates the excellent services of well trained optometrists just as does the rural population served by the optometrist of the small urban area.

It is true that it takes somewhat longer, and requires more capital, to start an optometric practice in a major city, but for those who can do so, the effort is well worthwhile. The young graduate who would like the experiences of city life should seriously consider the larger urban center as a place to establish his practice. With the present distribution of optometrists, many large cities offer an excellent opportunity for pleasant and lucrative practices.

CAREL C. KOCH.

TRANSACTIONS OF THE AMERICAN ACADEMY OF OPTOMETRY

A department devoted to announcements, reports, appointments, organization data, news, professional problems and ideals, as these relate to the Academy.

COMING ACADEMY MEETING IN ROCHESTER, NEW YORK

The 1952 annual meeting of the American Academy of Optometry will be held in Rochester, New York, December 6, 7, 8, and 9, 1952. The convention will headquarter at the Hotel Seneca.

Registration will start promptly at 8:30 a.m., Saturday, December 6. President Simmerman will call the session to order at 10:00 a.m. and the scientific and clinical portion of the program will start promptly thereafter. The Chairman of the Papers and Program Committee is Dr. Meredith W. Morgan, Jr. The Academy Sections dealing with aniseikonia, contact lens fitting, industrial optometry and orthoptics will meet as usual from 9:00 a.m. to 11:00 a.m. on December 7, 8, and 9. The annual formal round table dinner of the Academy will take place at 7:30 p.m. on December 8.

Members of the Academy should now make their hotel reservations for this annual meeting direct to the Hotel Seneca, Rochester, New York.

NEW MEMBERS OF THE ACADEMY

The following optometrists, educators and scientists were elected to Fellowship in the American Academy of Optometry during the past quarter. The Academy welcomes these new members.

Dr. Peter Abel, Kurfurstendamm 210, Berlin, W. 15, Germany.

Dr. Morton Abram, Chicago College of Optometry, 1849 Larrabee St., Chicago, Illinois.

Dr. Maurice E. Barre, R. O., 93 Dunlop St., Barrie, Ontario, Canada.

Dr. G. M. Belanger, R. O., 45 Rideau, Suite 302, Ottawa, Ontario, Canada.

Dr. C. Roger Brissman, 229 N. Laural Ave., Ontario, California.

Dr. M. Kirschen, P. O. Box 1465, Modesto, California.

Dr. Arthur Layton, P. O. Box 342, Paradise, California.

Dr. Angus M. MacLeod, Waterland-Breslauer Bldg., Chico, California.

Lt. Albert Eugene Mibeck, MSC, USN. Naval Medical School, Department of Optometry, National Naval Medical Center, Bethesda, Maryland.

Dr. William C. Moeny, 713 Main St., Alamosa, Colorado.

Dr. John N. Schoen, P. O. Box 441, Owatonna, Minnesota.

Dr. Robert H. Sternberg, Chicago College of Optometry, 1849 Larrabee St., Chicago, Illinois.

Dr. Edward Richard Tennant, Chicago College of Optometry, 1849 Larrabee St., Chicago, Illinois.

CURRENT COMMENTS

Virginia Huck
Editorial Assistant

Optometrists will confer a favor by sending news items of general interest for this department: such as relate to new instruments, clinical techniques, education, visual health and optometric legislation and organization.

FEINBERG HEADS N.I.C.O.

The appointment of Dr. Richard Feinberg to the presidency of Northern Illinois College of Optometry gives this school a man exceedingly well equipped for his post. Dr. Feinberg first came into national prominence in optometry when he served as supervisor of the eye service section of the Sperry Gyroscope Company, Great Neck, New York, at the start of World War II. Here, he and his colleagues made optometric history in this specialized field of industrial eye care. At one

time, the Sperry Gyroscope Company employed more than 30,000 people, all of whom were served in one way or another by this section. Feinberg's work was so successful that a review of his activities was presented in the proceedings of a conference on industrial ophthalmology published by the National Society for the Prevention of Blindness in 1945.

Dr. Feinberg left the Sperry Gyroscope Company to get his Ph.D. from Purdue University, after which he spent several years at Pacific University where he became dean of the College of Optometry. A year ago, he transferred to N.I.C.O. and served as executive vice president until his recent appointment to the presidency.

The staff of this publication wishes him the best of success in his new duties.

N.I.C.on.E.C. CHAIRMAN RESIGNS

The Journal learned last month with regret that Dr. Avery D. Prangen, senior ophthalmologist of the Mayo Clinic, Rochester, Minnesota, has, because of ill health, withdrawn from all of his many fine extra-curricular activities in the field of eye care. For optometry, as well as ophthalmology, this is an acute loss. Dr. Prangen was chairman of the National Interprofessional Committee on Eye Care and instrumental in the original organization of this committee because of his sincere desire to bring about improved interprofessional relations in the patient's best interest. The Journal joins his many friends in wishing Dr. Prangen speedy and lasting improvement in his health.

EYE CARE GROUP TO MEET IN OCTOBER

The National Interprofessional Committee on Eye Care will hold its regular fall meeting in Chicago at the Palmer House on November 12. This meeting will be held during the annual sessions of the American Academy of Ophthalmology and Otolaryngology. On the agenda will be the election of a new chairman to take the place of Dr. Avery D. Prangen who has resigned due to ill health. The meeting will be called to order by Leslie Myers, vice chairman of the committee.

CHECK!

Marty Topaz who edits the *Optometric Weekly* and who is also editor of the *Optical Index* (the leading publication for opticians), makes a telling point in the August issue of the *Optical Index* in an article entitled, "Put Down That Knife." Marty points out that optical industry people make a big mistake if they don't carefully read the journals which serve the eye professions. He points out that wholesalers and

laboratory men will do a better job if they know what the professions really want, and one of the best ways of finding this out is in the columns of the eye journals.

SCHOOL FREE EYE CLINIC GROWS

In March of this year, Earl A. Dimmeck, Superintendent of Schools at Pittsburgh, presented a report before the National Society for the Prevention of Blindness at its annual meeting. Superintendent Dimmeck reviewed the steps taken in establishing an eye clinic in the School Administration building in Pittsburgh in 1939. The clinic is tax supported. It was initially started to provide examinations and refractions for indigent students. Later a section for orthoptic treatment was added. It is significant to note that these orthoptic treatments are provided by a nurse who works under the supervision of the ophthalmologist who does the refractions. The report indicates that at present, not only are indigent children served at this clinic, but also the clinic works on other students as well who are referred to the clinic for diagnostic opinion. When found, no eye pathology is treated at the clinic, these cases being referred to hospital dispensaries. Since its formation in 1939, 17,000 students have been refracted and 9,500 orthoptic treatments have been provided. Inasmuch as no pathology is treated in this school clinic it becomes essentially a refractive activity with the optometrists conspicuous by their absence.

HOUSTON UNIVERSITY COLLEGE OF OPTOMETRY NEEDS BOOKS

Dr. Charles Stewart, dean of the new College of Optometry of Houston University has issued a request for contributions of optometric text books and reference material which could be used by his students. The library also needs a complete set of the *American Journal of Optometry and Archives of American Academy of Optometry*, and is prepared to buy these if necessary, if a set can be located.

The recently-established college will offer its first courses of a five-year curriculum this fall. Faculty members include Sara E. Huggins, Ph.D. anatomy; Donald C. Kroeger, Ph.D., physiology; H. M. North, M.A., physics, and Ruby Percival, B.S., bacteriology.

MRS. STONE LEAVES INDIANA ASSOCIATION

We were sorry to see Mrs. Florence Stone leave her public relations job with the Indiana Optometric Association. Perhaps we're prejudiced because she's a sorority sister (Theta Sigma Phi, honorary journalism sorority), but we think she did a splendid job on the *Indiana Optometrist* and her other public relations assignments. We hope the new set-up in Indiana will be just as effective.

OPTICIANS' GUILD RECRUITING NEW MEMBERS

In the current issue of *Guildcraft*, we note the Guild of Prescription Opticians of America is conducting a drive for membership. Until recently, the major membership of the Guild was in New England and along the Atlantic Seaboard. The elimination of retail dispensing by those laboratories owned and controlled by ophthalmic manufacturers permitted hundreds of dispensing opticians to open their own shops in all parts of the country—it is this group from which the Guild is recruiting. Optometry watches this activity with interest, as in the past it has had a rather annoying time with some Guild members. It is hoped that the activities of the National Interprofessional Committee on Eye Care will help mitigate friction between opticianry, as evidenced through some Guild members, and optometry.

A.O.A. PUBLIC RELATIONS ROUNDTABLE SET FOR NOVEMBER

The American Optometric Association will hold its second public relations roundtable at the Hotel Roosevelt, Pittsburgh, Pennsylvania, on November 23 and 24. At the first roundtable last year, it was voted to make this an annual event. Dr. Elmer M. Soles is director of the A.O.A. department of public information.

This year's program will include lectures by well known speakers (to be announced), a session on techniques for getting newspaper, radio, and television publicity, and how to conduct a forum.

The A.O.A. would like to have interested organizations notify them now if they plan to send a representative to the roundtable. Write Dr. Soles, Jenkins Building, Pittsburgh 22, if you're going. Every day brings constant reminders of the need for effective public relations for optometry—the roundtable is one means.

NEWS BRIEFS

Dr. Charles Sheard, Rochester, Minnesota, has been chosen dean of the Los Angeles College of Optometry to fill the vacancy left by Dr. Henry Hofstetter. (See this month's editorial.) . . . American Optical Company expects its new Southbridge research laboratory, now being constructed on the banks of the Quinebaug River, to be ready by November 1. The L-shaped building will contain 25,000 square feet of floor space. . . . The 13th annual Middle Atlantic Optometric Congress will be held October 26-28 at the Roosevelt Hotel, Pittsburgh. The clinical aspects of the visual care of children will be the congress theme. . . . Southeastern Educational Congress is scheduled for May 10-12, 1953, at the Seelbach Hotel, Louisville, Kentucky, according to a recent announcement from the publicity committee. . . . Don Wilschutz of

Marshalltown, Iowa, was chosen winner of the $1,500.00 Iowa Optometric Association scholarship to Chicago College of Optometry. Scholastic ability, personality and integrity were the criteria for judging applicants. . . . Some U.S.A. companies are now promoting a French spectacle frame, Amor. It was introduced in this country in May, 1952, after proving popular in France—Amor Ophthalmic Corporation claims one out of every three frames sold in France is an Amor. . . . Dr. Joe M. Ashmore, 702 Andrews Building, Spartanburg, South Carolina, is now handling the secretarial duties of the American Optometric Foundation. . . . The North Central Optometric Congress, scheduled for Minneapolis September 28-30, will feature a program on problems of children's vision. Speakers include Dr. Anita Eberl, Milwaukee, Dr. Nathan Robbins, Brooklyn, New York, Dr. Wayne Getman, Sioux City, Iowa, and Dr. A. M. Skeffington.

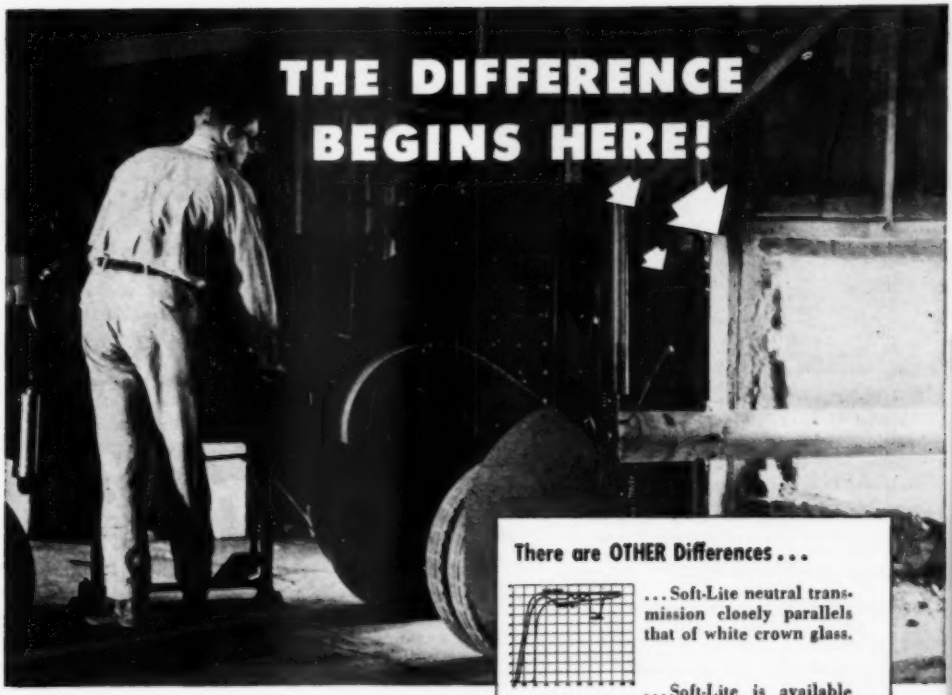
NEW DEVELOPMENTS BY AMERICAN OPTICAL COMPANY

Three new technical developments in the visual care field have been announced by the American Optical Company—a new type occlusion lens to aid in straightening crossed eyes, an instrument to improve detection of retinal breaks, and an eye-exercising device for home use.

The new partial occluding lens is designed to help correct eye deviations and aid the patient in achieving normal binocular vision. The lenses can be tinted to balance the color of regular lenses, making them look like an ordinary pair of glasses.

The instrument designed for better detection of retinal ruptures or detachments has been named the stereoscopic indirect ophthalmoscope. It produces in mid-air a three-dimensional inverted image of the retina for observation, permitting a better view of the retina than possible heretofore. This instrument was developed at the Retina Service of the Massachusetts Eye and Ear Infirmary, Boston, in cooperation with A. O. Company.

A. O. Company's new eye-exercising device for home use, called a bar separator, is designed to aid in the correction of children's eye abnormalities. With instructions from the doctor, parents can supervise home training as an adjunct to professional training. This home visual trainer provides eye exercises to aid in achieving binocular vision in squint patients and overcoming other eye deficiencies such as poor acuity, inadequate fixation and improper hand and eye coordination. The bar separator was devised by A. O. Company in cooperation with Miss Frances Walraven of Atlanta, Georgia, former president of the American Association of Orthoptic Technicians.



The difference between Soft-Lite and so-called "tinted" lenses begins right at the source – the materials from which glass itself is made.

Soft-Lite glass is made, according to a 44-year-old formula, from the finest materials obtainable. These materials differ radically from those used in common tinted lenses; the ingredient providing Soft-Lite's unique absorptive properties is more costly and more difficult to process. And Soft-Lite glass is made in a plant devoted exclusively to the making of ophthalmic and optical glass – that of Bausch & Lomb.

There are OTHER Differences . . .

. . . Soft-Lite neutral transmission closely parallels that of white crown glass.

. . . Soft-Lite is available in five degrees of absorption, from 14.8% to 64%.

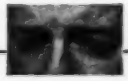
. . . Soft-Lite Lenses are available in Orthogon curves.

. . . Soft-Lite is available in most Bausch & Lomb lens forms.

The Difference can be SHOWN . . .

. . . in the Soft-Lite Comparator. Both Soft-Lite and white crown glass show the same transmission characteristics, while ordinary tinted lenses do not.

Soft-Lite Lenses **make the difference!**

Some eyes can take it . . .  **some eyes cannot!**

he's lost what he paid for, Doctor...

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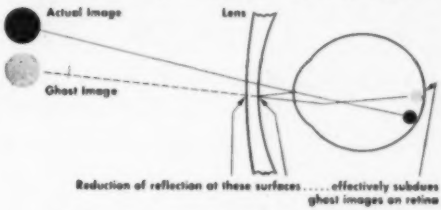

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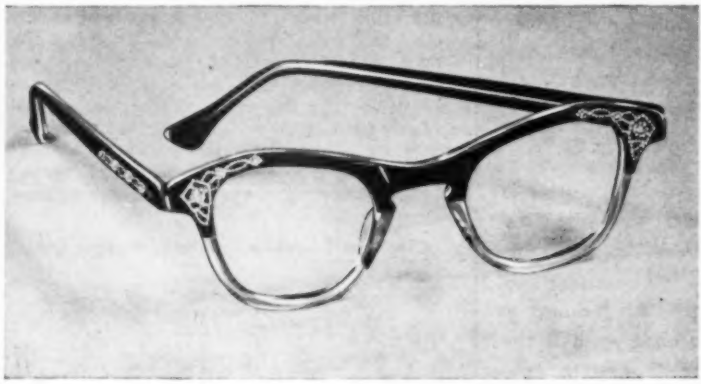

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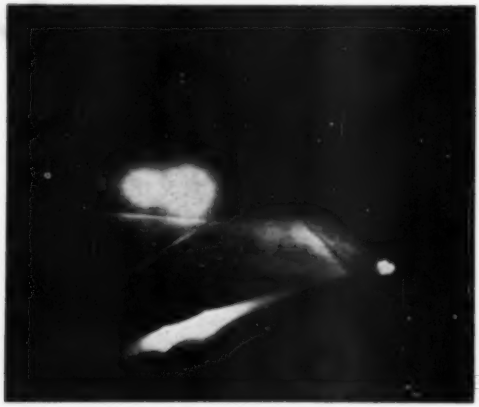

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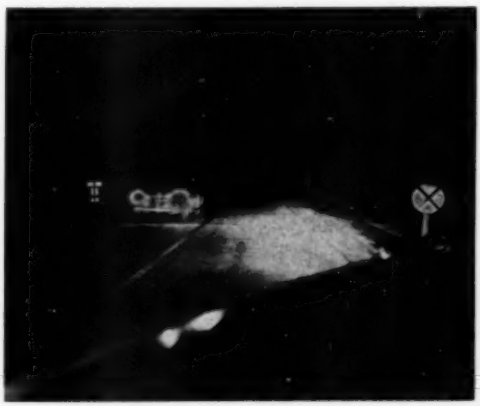

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